PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

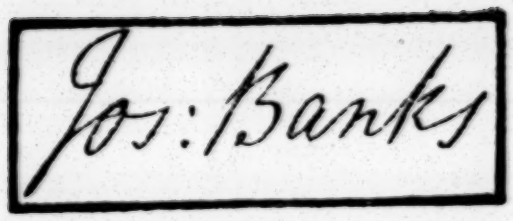
IN MANY

Conſiderable Parts of the WORLD.

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Jos: Banks

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PART

PART II.

For the YEAR 1760.

XL. *A Letter to the Honourable* J. Th. Klein, *Secretary to the City of* Dantzick, *from Mr.* Peter Collinſon, *F. R. S. concerning the Migration of Swallows.*

Read March 9, 1758.

I Do not know which to admire moſt in my dear friend Klein, his learning, or his judgment, in compiling ſo many ingenious and inſtructive books in natural hiſtory.

But I muſt beg leave to diſſent from my learned friend, in an article he takes great pains to eſtabliſh; which is, that ſwallows are not birds of paſſage; but, at the time of their going away, retire under water, and live therein, all the winter. This I cannot comprehend, being ſo contrary to nature and reaſon; for as they cannot live in that ſtate, without ſome degree of breathing, this requires the circulation of the blood, however weak and languid. Now, as reſpiration is abſolutely neceſſary for circulation, how is it poſſible to be carried on for ſo many months under water, without the riſque of ſuffocation?

Beſides, if ſo remarkable a change was intended, the great wiſdom of the Almighty Creator would, undoubtedly, be ſeen in ſome particular contrivance, in the ſtructure of the organs of the heart of this bird, to enable it to undergo ſo very remarkable a change of elements: and my learned friend has not attempted to ſhow any thing of this nature, in order to confirm his ſyſtem.

An eaſy experiment may throw ſome light on this doubtful affair——At the time of their going away, take a ſwallow, and confine it in a tub under water: if it remains there for a week or two alive, without any remarkable inconvenience, then there may be ſome probability for its continuing ſo many months in that ſtate.

The concluſions, that are drawn from ſome of the tribe of inſects ſubſiſting under water, are far from being concluſive, to found an analogy upon; as inſects differ from other animals in ſo many particulars, that very little or nothing can be concluded, or inferred, of the one, from what we obſerve in the other.

Towards the end of September, the ſwallows aſſemble on the reeds in the iſlands in our river Thames, and have, no doubt, ſo done for ages paſt; and yet I never heard, or read, of any fiſherman, or other perſon, that has ever found in the winter months a ſwallow under water, in a torpid living ſtate; for if ſuch a marvelous thing had ever happened, it would have been ſoon communicated to the public. Beſides, as theſe iſlands of reeds and willows are annually cut down, for ſeveral uſes, and yet not a ſwallow has been diſcovered in his aquatic abode; and, conſider-

ing

ing the multitudes I have ſeen on theſe reeds and willows in the autumn; if they took their winter's reſidence under water, it is moſt reaſonable to think, in a river ſo frequented, and in ſo long a courſe of years, ſome would have been found in that ſituation. Another circumſtance I muſt add; in great towns remote from water, where rivers and reeds are not near, I have frequently obſerved that, a little before the ſwallows depart, they, every morning early, gather together on the roofs of large houſes, expoſed to the morning ſun: this they daily do for ſome time, to collect themſelves, before they take their flight.

Next, to confirm my opinion, that the migration of ſome ſpecies of ſwallows is certain, I think I have ſome undoubted proofs.

I have often heard Sir Charles Wager, firſt lord of the admiralty, relate that, in one of his voyages home, in the ſpring of the year, as he came into ſoundings in our channel, a great flock of ſwallows came and ſettled on all his rigging: every rope was covered, they hung on one another like a ſwarm of bees; the decks and carvings were filled with them; they ſeemed almoſt ſpent and famiſhed, and were only feathers and bones; but, being recruited with a night's reſt, they took their flight in the morning.

Capt. Wright, a very honeſt man, whom I could depend on, told me, the like happened to him, in a voyage from Philadelphia hither.

But a yet ſtronger confirmation of the ſwallows being birds of paſſage is the obſervation in Mr. Adanſon's hiſtory of Senegal, lately publiſhed; which is, as near as may be literally tranſlated, from the author's own words; *viz.* "The ſixth of the ſame month,

" (October) at half an hour paſt ſix in the evening,
" being about fifty leagues from the coaſt, (between
" the iſland of Gorea and Senegal) four ſwallows
" came to take up their night's lodging on the ſhip,
" and alighted on the ſhrouds. I eaſily caught all
" four, and knew them to be the true European
" ſwallows. This lucky accident confirmed me in
" the opinion I had formed, that theſe birds paſs the
" ſeas, to get into the countries of the torrid zone, at
" the approach of winter in Europe; and, to that
" purpoſe, I have ſince remarked, that they do not
" appear at Senegal but in that ſeaſon. A circum-
" ſtance no leſs worthy of note is, that at Senegal
" the ſwallows do not build neſts, as in Europe; but
" lie every night by pairs, or ſingle, in the ſand upon
" the ſea-ſhore, where they rather chuſe to fix their
" habitation, than up in the country." *Hiſt. de Senegal, p.* 67.

This obſervation (as it comes from a profeſſed naturaliſt, and one, who went into thoſe countries on purpoſe to collect what was curious in that way) ſeems to put the matter out of doubt; and the hear-ſay ſtories of ignorant peaſants and credulous people are by no means to be put in competition with it.

I have, for many years, been very watchful in taking notice of the times when the ſwallows leave us, and have twice ſeen them undoubtedly taking their flight. At two different years, on the 27th and 29th of September, walking in my garden at noon, on very clear ſunſhiny days, and looking up into the ſky, at a very great height I diſtinctly ſaw an innumerable number of ſwallows, ſoaring round and round, higher and higher, until my eyes were ſo

pained

pained with looking, that I could no longer diſcern them.

But, as my friend Klein ſeems to be ſo poſitive, that the hirundo riparia, or ſand martin, at the approach of winter, retires into the holes, in which that ſpecies breed up their young, and made their ſummer's reſidence, and there paſs that cold ſeaſon in a dormant ſtate, as ſnakes, lizards, and ſome other animals do, I have been the more ſollicitous to come at the truth. But as theſe ſandy precipices, in which theſe martins build, are moſtly inacceſſible, ſome years have paſſed, before I could find a ſituation where the experiment could be fairly made, without difficulty or danger. Such a ſand-hill I found in the pariſh of Byfleet in Surry. The clergyman being my friend, and well qualified to make the experiment, at my requeſt, was ſo obliging to undertake it. I ſhall give his letter to me, in his own words.

" Byfleet, October 22, 1757.

" Dear Sir,

" I took a ſquare of about twelve feet, over that
" part of the clift where the holes were thickeſt,
" which, in going down from the ſurface, I judged
" would take in about forty holes. I ſet to work,
" and came to the holes; but found no martins,
" nothing but old neſts in the furtheſt end of the
" holes, which were from a foot and half to two
" feet and half deep from the entrance. We care-
" fully ſearched forty holes, but found no birds;
" but at leaſt thirty of them had neſts. The paſſage
" to them was very near in a ſtraight horizontal line;
" the neſt was ſunk about an inch and half below
" the level of the paſſage; the materials next the

" bottom

" bottom were ſtraws, then coarſe and fine graſſes;
" the whole ſtructure of no great elegance. The few
" eggs, that were left behind, were of a clear un-
" ſpotted white, the ſize of a robbin-red-breaſt's."

This fair trial being made by a gentleman of veracity and ability is very concluſive; for it certainly proves, that the ſand martins do not take up their winter abode in their ſummer dwellings. Therefore, there is ſufficient reaſon to believe, from the before-recited obſervations *on the common ſwallows*, and *this* ſo recently made on the *ſand martins*, that they are all birds of paſſage.

Additional Remark.

There are four diſtinct ſpecies of birds, that go under the general name ſwallow; *viz.* the ſwift or black martin; 2. the ſwallow, that builds in chimneys; 3. the martin, that builds againſt houſes; 4. the ſand martin, that builds in ſand-banks. I hope, that I have clearly proved, that ſome of theſe ſpecies are birds of paſſage. But ſome of my friends aſſert, that they paſs the winter in clifts or caverns of the earth, in banks or precipices. What is much to be regretted is, that the gentlemen were not curious enough to diſtinguiſh the particular ſpecies, which they found in a torpid ſtate. Monſ. Adanſon, in his account of Senegal, has omitted this. So that nothing certain can yet be pronounced, which ſpecies ſtays, or which goes.

XLI. *Obſervations on the Comet ſeen in* January 1760; *by* James Short, *M. A. F. R. S. In a Letter to the Rev.* Tho. Birch, *D. D. Secretary to the Royal Society.*

Dear Sir, Surry-Street, 10 Jan. 1760.

Read Jan. 10, 1760.

A Comet has made its appearance near the conſtellation of Eridanus, a little to the weſtward of Orion. Laſt night I took its tranſit over the meridian, and likewiſe its declination. Its nucleus is ſmall, ſubtending an angle of not more than 5 or 6 ſeconds, but very viſible thro' a two feet reflector magnifying about 70 times. Its motion is to the weſtward, with a conſiderable velocity, ſeemingly about 2 degrees in a day; for, about an hour and a half after I had taken its tranſit, I judged it had advanced about 10 or 12 minutes; which I find to be about the rate of the great comet, when it firſt was ſeen in the end of the year 1743. This comet is very viſible to the naked eye, tho' I could perceive nothing of a tail; and therefore I conclude it is going down to the ſun.

	Mean time.
Comet paſſed the meridian 9th January 1760, at - - - - - - - - - -	$9^h\ 8'\ 53''$
Its declination ſouth - - - - - -	$2°\ 15'\ 0''$
Rigel Orionis paſſed the meridian at -	$9^h\ 47'\ 49''$

I am, Dear Sir,

Your moſt humble ſervant,

Ja. Short.

XLII. *Obſervations on the ſame Comet; by the Rev.* John Michell, *M. A. Fellow of* Queen's College *in* Cambridge. *In a Letter to Mr.* James Short, *F. R. S.*

Dear Sir,

Read Jan. 10, 1760. THough in all probability you will have ſeen the comet, which is now apparent; yet, leſt you ſhould not, I was not willing to omit this opportunity of informing you of it. I received a meſſage laſt night from Dr. Maſon, who, as far as I find, was the firſt here, that diſcovered it. We did not ſuſpect at firſt, that its apparent motion was ſo great, as it appeared to be on examination; or elſe ſhould have taken more obſervations. Very luckily, however, I took its diſtance from four principal ſtars, between a quarter and half an hour after nine, which was as ſoon as I ſaw it; and, about an hour and a quarter after, finding that it had manifeſtly moved, to the naked eye, I took other obſervations of it, every quarter of an hour, or twenty minutes, till very near two o'clock, when it ceaſed to be viſible, on account of its being too low in the mud.

The firſt obſervations gave its diſtance from κ Orionis, 3° 29′; from Rigel, 11° 46′; from Betelgeuſe, 17° 10′; and from Syrius, 12° 56′. All theſe obſervations were made between a quarter and half an hour paſt nine, and, as well as I could recollect, in the order I have ſet them down; which, not imagining

gining it was material, I had not been careful to mark to ſingle minutes. At $1^h\ 22'$, its diſtance from Rigel was $7^\circ\ 6'$; at $1^h\ 24'$, from Betelgeuſe $15^\circ\ 53'$; and at $1^h\ 36'$, its diſtance from Sirius was $17^\circ\ 36'$.

This will be ſufficient for you to find its place by. The obſervations, together with above twenty others, were made with a little Hadley's quadrant, and may moſt of them, I believe, be depended on to about two or three minutes; but ſome perhaps may err four or five minutes.

I am, Sir,

Your obedient ſervant,

Cambridge, Jan. 9, 1760. John Michell.

XLIII. *An Account of the ſame Comet*; *by* Nicolas Munckley, *of* Lincoln's-Inn, *Eſq*;

Read Jan. 17, 1760.

JAnuary 9, 1760, I obſerved what appeared to me to be evidently a comet, weſt of the conſtellation of Orion, or (to ſpeak more aſtronomically and exactly) over the two ſtars marked μ and ν in the river Eridanus, but nearer the latter than the former; right aſcenſion, about 66 deg. declination, about 3 deg. S. It was ſomething dimmer and larger than either of theſe ſtars; and through a teleſcope, appeared magnified, and ſurrounded with a broad, faint, ill-defined hazineſs, like the laſt comet, ſuch as plainly diſtinguiſhed it from any

thing elſe in the heavens. It ſeemed, even between the times I obſerved it that evening, to have a ſenſible motion towards the north-weſt (*i. e.* nearer the zenith, and contrary to the order of the ſigns): for though at firſt I apprehend its place to be as above, on reviewing it, two or three hours afterwards, it was more over the ſtar *ν*, and a little to the weſtward of it.

This phænomenon was ſeen the night before (*viz.* the 8th, when I was in London), by two or three perſons, who mentioned it to me: but though they particularly took notice of it as different from other ſtars, they were not aſtronomers enough to give any exact account, either of its place or appearance.

The evenings following the 9th were cloudy, till the 13th, which, though far from being very clear, allowed me however, at times, to ſee the conſtellation Orion, and weſtward of it, as far as Cetus, *&c.* I ſaw particularly the ſtars *μ* and *ν*, mentioned before, and the places above-referred to over them; but I ſaw nothing any more of the comet.

Hampſtead. Nicolas Munckley.

XLIV. *An Account of the ſaid Comet; by Mr.* Mark Day.

SIR, Lowick, near Thrapſton, Northamptonſhire.

Read Jan. 17, 1760. I Had the pleaſure laſt night, about five o'clock, to obſerve a comet in the ſouthern hemiſphere, near the northmoſt extremity of the river Eridanus, tending its courſe towards Pegaſus, and, near as I can gueſs, it will croſs the ecliptic about 20° in Aries. It moves one degree in leſs than one hour and half; but ſeems too haſty to give the aſtronomers leave to make many obſervations upon it, unleſs the weather prove favourable. If there has been any accurate obſervation thereupon, I would gladly know; but if it has not been taken notice of, then I hope the above will be acceptable from,

SIR,

Your very humble ſervant,

Jan. 10, 1760. Mark Day.

It paſſed the meridian about 9.

XLV. *Of the vitriolic Waters of* Amlwch, *in the Iſle of* Angleſey; *with occaſional Remarks on the* Hartfell *Spaw, deſcribed in the firſt Volume of the* Edinburgh *Eſſays and Obſervations phyſical and literary, and in the* 49*th Volume of the Philoſophical Tranſactions, and their Compariſon with other Waters of the ſame Claſs.* *By* John Rutty, *M. D.*

Read Jan. 17, 1760.

THE truly vitriolic waters, altho' by much the ſtrongeſt of the chalybeates, are ſo rare in compariſon of the ordinary chalybeates, that we have ſcarce any accounts of them in the memoirs of the learned, nor any particular hiſtories of ſuch waters, until of late; and therefore, it is preſumed, that an additional account of a certain notable water of this claſs may not be deemed unworthy of being preſerved.

Such a water is that of Amlwch, ſituate on Traſklwyn mountain, in the pariſh of Amlwch, in the Iſle of Angleſey, which was ſent me by Ambroſe Lewis, of Beaumorris, having been bottled May 31, 1757, and arrived in Dublin June the 3d following.

It appears, by the hydrometer, to be as light as diſtilled water, notwithſtanding its ſtrong impregnation.

It is of a ſubacid taſte, and very nauſeouſly vitriolic; a laſting impreſſion of that ſort continuing in the throat giving ſuſpicion of copper, of which, however,

however, it exhibits no evidence, by any degree of the hue of that metal imparted to polished knives immerſed in it, nor of ſulphur, by diſcolouring ſilver. It retained the above-mentioned taſte, on being expoſed ſeveral days in an open veſſel; quite otherwiſe than happens to our ordinary chalybeates.

It curdled with ſoap: it alſo curdled equal parts of milk, exhibiting a pretty clear whey.

With ſpirit of hartſhorn, ſpirit of ſal ammoniac, and the ſolution of potaſhes, it exhibited ochreous and green grumes, as the martial vitriol.

Its appearances with galls, and other auſteres, were very ſingular, and worthy remark: for tormentil roots gave it only a dilute ink colour, ſoon fading; and green tea a dark dun colour, on ſtanding. It had not blackened the corks, except perhaps one out of ſix, that ſtopped the bottles ſent me; and when the water was firſt poured out, it ſtruck no more than a ſlightly bluiſh tincture with galls; which tincture, on ſtanding all night, became like a dilute ink; but in a glaſs expoſed thirty-two hours, the galls ſtruck the dilute ink colour ſooner: and in ſome of the water, which I left expoſed four days, and in ſome of it, which I left in a phial corked, but only three quarters full, three weeks, the effect was very different; for to each portion of water ſo expoſed, as I have juſt now mentioned, the galls imparted a moſt beautiful bright ſky blue; which blue tincture, a little ſpirit of vitriol inſtantly deſtroyed.

Hence we may ſee the fallacy of truſting to a few appearances, and the danger of raſh concluſions, without repetitions of experiments and obſervations; which, if they had been omitted, I had been led to

conclude this water to be but ſlightly impregnated with either a chalybeate or vitriolic principle; with which laſt it is, however, aſſuredly ſtrongly ſaturated, altho' the ordinary taſte with galls does not diſcover it clearly by the blue tincture, until the diſſolving acid is partly exhaled.

The ANALYSIS.

It depoſits an ochre, which ſerves for painting.

It alſo depoſits in the bottles a ſediment partly of a red and yellowiſh colour, and partly white and raggy; which ſparkled, and ſmelt ſtrong, on the red hot iron.

Two pounds eleven ounces yielded 16 grains (*i. e.* a gallon 49 grains) of a light green ſediment, of an acid ſmell, and of an highly acid, vitriolic, and nauſeous taſte.

It ferments ſtrongly, both with ſolution of potaſhes, and with ſpirit of ſal ammoniac; and ſeparates a green and ochreous matter with the laſt.

Galls added to its dilutum in diſtilled water turned it of a deep blue; the characteriſtic of martial vitriol, to which it alſo agrees in the experiments of the laſt paragraph.

It produced ſome degree of coagulation with albumen ovi; and ſome ſlight opacity, and ſmall grumes, with ſaliva.

It turned of a brown greeniſh colour with ſyrup of violets.

It was not attracted by the magnet, until roaſted in the crucible; and then it was ſtrongly attracted, and turned as red as minium.

From

From the above experiments, it appears, that Amlwch water is ſtrongly impregnated with an acid martial vitriol.

It kills all the fiſh in its paſſage.

It has ſometimes been drunk; but cannot be borne in a greater doſe than half a pint, unleſs diluted with common water, being otherwiſe vomited up.

It cures the mange in horſes, and the itch in men, by bathing.

From the above account of the Amlwch water, it appears evidently to agree to that of the Hartfell, above-mentioned, and deſcribed in the Edinburgh Eſſays, and in the Philoſophical Tranſactions, even in certain diſtinguiſhing characters common to both, and in which they differ from our ordinary chalybeate waters; *viz.*

1. In the acid and vitriolic taſte, which, moreover, they retain, when long kept, and at a diſtance from their fountains; and even upon boiling, yielding an acid vitriolic ſalt, on exhaling to dryneſs, which the common chalybeates never do, but loſe their ſtrength by a ſmall degree of heat.

2. In the blue tincture, which they give with galls; another diſtinguiſhing character of Engliſh vitriol, of which a weaker ſolution, like the common chalybeates, gives only the purple colour with galls.

3. In exhibiting green clouds, or grumes, with oil of tartar, like the martial vitriol.

The ſame, or like appearances, are exhibited by the Shadwell water; by another at Swanzey in Wales; and, upon a late diligent ſearch into the waters of this kingdom, by thoſe of Kilbrew, in the county of Meath; of Ballymurtogh, in the county of Wicklow;

low; and of Croſs and Coſhmnore, in the county of Waterford, and ſome others: and, to conclude, as cryſtals of martial vitriol have been demonſtrated in ſeveral of them, I ſhall not heſitate to pronounce them *acid vitriolic waters*; which waters, as they are new in practice, and different in operation and effects from the common chalybeates, I apprehend it will be worth while to endeavour to place them in a more conſpicuous point of view, which the hiſtories lately given of the Britiſh waters of this claſs above-mentioned, compared to our own, will enable us, in ſome meaſure, to do.

There are, indeed, in theſe waters different degrees of acrimony; for tho' moſt of them are ſo acid as to curdle milk, yet this is not altogether univerſal; and tho' in many of them the acid is ſo far developed in their ſolid contents, as to ferment with alcali's, this appearance is not always conſpicuous; from whence one would imagine, that the milder ſort might be uſed with more freedom, or leſs danger: and yet it is certain, that one of the ſharpeſt of them all, *viz.* ours at Kilbrew, in the county of Meath, hath been taken inwardly, with amazing ſucceſs, in ſome very ſtubborn caſes.

Yet, on the other hand, it hath been obſerved, that even the German ſpaw ſometimes hath proved too irritating in ſome tender conſtitutions, where our ordinary milder chalybeates have ſucceeded well; and we are otherwiſe informed, by an accurate obſerver, that, in ſome tabid caſes, particularly that called the galloping conſumption, the mildeſt and lighteſt of our own chalybeates, and even altho' blended with milk, have

have been found to increaſe the hectic heats and tenſion of the pulſe.

Now, this obſervation ſeems not eaſily reconcileable to another of Dr. Horſeburgh, in the place above-mentioned, on the Hartfell ſpaw (a much ſtronger and harſher chalybeate than either the German ſpaw, or any of our ordinary chalybeates), *viz.* that it hath actually been given, with notable ſucceſs, from half a pint to a pint a day, in conſumptions of the lungs, far advanced, even attended with hectic heats and night ſweats.

So memorable a fact, in the cure of a deplorable diſeaſe, deſerves attention; and the Scotch phyſicians in that neighbourhood are called upon to corroborate it by further obſervations; as how long thoſe cures ſtood, and how far they may have been confirmed by the like ſucceſs in ſimilar caſes; whether uſed with or without milk; and laſtly, whether, as an acid auſtere medicine, they may cool, correct, and give a better conſiſtence, in a colliquative ſtate of the blood; ſeems well to deſerve further inquiry, and that the reſult ſhould be communicated for the public utility.

There had indeed formerly obtained a general prejudice againſt the uſe of the ordinary chalybeates in diſeaſes of the lungs; but, at length, experience hath convinced us, not only of their ſafety, but uſefulneſs, and good effects, eſpecially when tempered with milk, in many of thoſe caſes. And moreover, it is but doing juſtice to our acid vitriolic waters, to acknowlege, that the empirical trials made on them by the giddy vulgar have been frequently ſuch, as demonſtrate, not only their ſafety, but even powerful effects in other rebellious diſorders; as, particu-

larly, our Kilbrew water (one of the ſharpeſt and moſt ſtrongly ſaturated with martial vitriol of all theſe waters yet diſcovered), in the notable cure of an aſcites, complicated with a jaundice, which I have elſewhere related; and I ſee no reaſon why phyſicians ſhould not, in this as well as other caſes, avail themſelves of the happy ſucceſs of ſuch caſual experiments.

In order, therefore, to promote a view of this kind; and, furthermore, as theſe vitriolic waters are better adapted for uſe than the ordinary chalybeates, as bearing carriage to remote places, and may be kept fit for uſe at all ſeaſons of the year, and are to be preferred in medical intentions, whenever the ſtrongeſt of the chalybeates are required, and can be borne; I ſhall here, from facts and obſervations made on the ſeveral waters of this ſort, which have fallen under my notice, give a ſhort ſketch of their general operation and good effects, as a foundation for further improvements.

Theſe waters, then, generally operate as an emetic or cathartic, or both; and have recommended themſelves, in external and internal uſe, as a powerful detergent, repelling, bracing, ſtyptic, cicatrizing, antiſcorbutic, and deobſtruent medicine, as hath appeared by the notable cures they have effected, not only by external uſe in inveterate ulcers, the itch, mange, ſcab, tetterous eruptions, ſcald head, and ſore eyes; but alſo by internal uſe in hot tetterous eruptions, dyſenteries, internal hæmorrhages, in gleets, the fluor albus, and diarrhæa, in the worms, agues, dropſies, and jaundice.

Such

Such hath been the ſucceſs, that has not unfrequently crowned the empirical uſe of theſe waters; which, though, in ſome of theſe caſes, it might undoubtedly have been better conducted in the hands of the prudent phyſician, may, however, ſuffice to convince us, that the vitriolic waters are a branch of the materia medica, not to be deſpiſed nor overlooked, in the cure of many ſtubborn chronical diſeaſes.

Dublin, 15 February, 1750.

XLVI. *An Account of that Part of* America, *which is neareſt to the Land of* Kamtchatka; *extracted from the Deſcription of* Kamtchatka *by Profeſſor* Kraſhennicoff, *printed at* Peterſburg, *in two Volumes*, 4to. *in* 1759; *and tranſlated and communicated by the Rev.* D. Dumareſque, *D. D. Chaplain to the* Engliſh *Factory at* Peterſburg.

CHAPTER X.

Of America.

Read Jan. 24, 1760.

ALthough we have no exact and circumſtantial accounts of America, which lies eaſt of Kamtchatka, for which reaſon, the deſcription of that country might be deferred to the time, that the voyage to America, at the Kamtchatka expedition, will be publiſhed; nevertheleſs,

for the ſake of regularity, and that the reader may ſtill have ſome notion of all the countries, that are in the neighbourhood of Kamtchatka, we are going to communicate here, what is collected in the memoirs of Mr. Steller, in different places.

The continent of America, which now is known from 52 to 60° of north latitude, extends from the ſouth-weſt to the north-eaſt, every-where almoſt at an equal diſtance from the Kamtchadalian ſhores, *viz.* about 37° longitude: for the Kamtchadalian ſhore, alſo, from the Kurilian Lopatka [the ſhovel *] to cape Tchukotſki, in a ſtrait line (except where there are bays and capes), lies in the very ſame direction. So that one has grounds to infer [from thence], that thoſe two lands were once joined, eſpecially in thoſe parts, where lies cape Tchukotſki: for, between that and the coaſt, that projects, which is found at the eaſt, directly over-againſt it, the diſtance does not exceed two degrees and a half.

Steller brings four arguments to prove this:

1. The ſtate of the ſhores, which, both at Kamtchatka and in America, are ragged [broken, cragged].

2. The many capes, which advance into the ſea, from 30 to 60 verſtes.

3. The many iſlands in the ſea, which ſeparate Kamtchatka from America.

4. The ſituation of thoſe iſlands, and the inconſiderable breadth of that ſea.

As to the reſt, we leave this to the conſideration [or judgment] of more ſkilful perſons: ſufficient it

* The words included between the [ ] are added by way of illuſtration, or elſe are, for the moſt part, the literal tranſlation of the Ruſs.

will

will be for us, barely to relate what was obſerved round about thoſe parts.

The ſea, which divides Kamtchatka from America, is full of iſlands, which [lying] over-againſt the ſouth-weſt end of America, extend [as far as] towards the ſtreights of Anian, in ſuch an uninterrupted ſeries [row, order], as the Kurilian iſlands do [as far as] towards Japan. That row of iſlands is found between 51 and 54° latitude, and lies directly eaſt; and it begins not farther than 5 degrees from the Kamtchadalian ſhore.

Steller thinks, that Company's-land is to be found between the Kurilian and the American iſlands (which many doubt of), if one [ſetting out] from the ſouth-weſt extremity of America, advances ſouth-weſt: for, in his opinion, Company's-land muſt be the baſe of a triangle [which it forms] with the Kurilian and the American iſlands; which ſeems not to be deſtitute of foundation, if Company's-land be rightly laid down on the maps.

The American land is in a much better ſtate, with regard to the climate, than the farthermoſt eaſtern part of Aſia, though it lies near the ſea, and has, every-where, high mountains, ſome of which are covered with perpetual ſnows; for that [country], when its qualities are compared to thoſe of Aſia, has, by far, the advantage. The mountains of [that part of] Aſia are, every-where, ruinous and cleft [broken]; from whence they have, long ſince, loſt their conſiſtency, they have loſt their inward warmth; upon which account, they have no good metal [of any kind]; no wood, nor herbs, grow there, except in the vallies, where is ſeen ſmall [bruſh-] wood and

ſtiff

ſtiff herbs. On the contrary, the mountains of America are firm, and covered, on the ſurface, not with moſs, but with fruitful earth [or mould]; and therefore, from the foot to the very top [of them], they are decked with thick and very fine trees. At the foot of them grow herbs proper to dry places, and not to marſhy ones; beſides that, for the moſt part, thoſe [plants] are of the ſame largeneſs and appearance, both on the lower grounds, and on the very tops of the mountains; by reaſon, that there is every-where the ſame inward heat and moiſture. But in Aſia, there is ſo great a difference between them, that, of one kind of plants growing [there], one would [be apt to] make ſeveral kinds, if one did not obſerve a rule, which holds generally, with regard to thoſe places [*viz.*], that in lower grounds herbs grow twice as large as thoſe on the mountains.

In America, even the ſea-ſhores, at 60° latitude, are woody; but in Kamtchatka, at 51° latitude, no place ſet with ſmall willows and alder-trees is found nearer than 20 verſtes from the ſea: plantations [or woods] of birch-trees, are, for the moſt part, at [the diſtance of] 30 verſtes; and, with regard to pitch-trees, on the river Kamtchatka, they are at the diſtance of 50 verſtes, or more, from its mouth. At 62°, there is no wood at Kamtchatka.

In Steller's opinion, from the afore-mentioned latitude of America, the land extends as far as 70°, and farther; and the chief cauſe of the above-ſaid growth of woods in that country is the cover and ſhelter it has from the weſt. On the other hand, the want of this [of wood] on the Kamtchadalian ſhores, eſpecially on the ſhore of the Penſhinian ſea,

doubtleſs, comes from a ſharp north wind, to which it is much expoſed. That thoſe parts, which lie from the Lopatka, farther to the north, are more woody and fruitful, is owing to cape Tchukotſki, and the land that has been obſerved over-againſt it, by which thoſe [parts] are ſheltered from the ſharp winds.

For this reaſon, alſo, fiſh comes up the rivers of America earlier than thoſe of Kamtchatka. The 20th of July, there has been obſerved a great plenty of fiſh in thoſe rivers; whilſt at Kamtchatka, it is then but the beginning of an abundant fiſhery.

Of berries, they ſaw there an unknown kind of raſberries, which bore berries of an extraordinary bigneſs and taſte. As to the reſt, there grow in that country black-berries [with ſeveral other kinds of berries, called in Ruſs, *jimoloſt*, *golubitſa*, *bruſnitſa*, and *ſhikſha*], in as great plenty as at Kamtchatka.

There are creatures enough, good for the ſupport of the inhabitants of thoſe parts; particularly ſeals, ſea-dogs, ſea-beavers, whales, * *canis carcharias*, *marmottes* [*marmotta minor*], and red and black foxes, which are not ſo wild as in other places, poſſibly becauſe they are not much chaced [hunted].

Of known birds, they ſaw there magpies, ravens, ſea-mews [† urili], ſea-ravens, ſwans, wild ducks, jackdaws, woodcocks, ‡ Greenland pigeons, and

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* In Ruſs, *akul*, or *mokoia*; in bigneſs, it is inferior to the whale; and it is like it in this, that it caſts no ſpawn, but brings forth young; upon which account, ſome reckon it a ſpecies of whale. Deſcript. p. 308. 1ſt vol.

† Uril, corvus aquaticus maximus, criſtaceus, periophthalmiis cinnabarinis, poſteà candidis.

‡ Columba Groenlandica, Batavorum, *kaiover*, vel *kaiour*.

* *mitcha-*

* *mitchagatki*, otherwiſe called northern ducks. But, of unknown birds, they obſerved more than ten ſorts, which it was not difficult to diſtinguiſh from European birds, by the livelineſs of their colours.

With regard to the inhabitants of thoſe parts, they are ſuch a wild people, as the Koriaki and Tchuktchi. As to their perſons, they are well ſet, broad and ſtrong ſhouldered. The hair of their head is black, and ſtrait, and they wear it looſe. Their face is tawny [brown], and flat as a plate; their noſe is flat, but not very broad; their eyes are as black as jet; their lips thick; their beard ſmall; and their neck ſhort.

They wear ſhirts with ſleeves, which reach lower than the knee; and they tye them up, with thongs of leather, below the belly. Their breeches and boots [which are made] of the ſkins of ſeals, and dyed with alder, much reſemble the Kamtchadalian. They carry, at their girdles, iron knives, with handles, ſuch as [are thoſe] of our boors. Their hats are platted [matted] of herbs, as with the Kamtchadalians, without a [riſing] top, in the ſhape of an umbrella; they are dyed in green, and in black, with falcon's feathers, in the fore-part, or with [ſome] herb, combed, as if it were a plume of feathers, ſuch as the Americans uſe about Braſil. They live upon fiſh, ſea animals, and the ſweet-herb, which they prepare after the Kamtchatka manner. Beſides this, it has been obſerved, that they have alſo the bark of poplar,

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* *Mitchagatka*, alca monachroa ſulcis tribus, circo duplici utrinque dependente. Anas arctica cirrata.

or of the pine-tree, dried, which, in caſe of neceſſity, is made uſe of as food, not only at Kamtchatka, but likewiſe throughout all Siberia, and even in Ruſſia itſelf, as far as Viatka; alſo ſea weeds made up into bundles, which, in look and in ſtrength, are like thongs of raw leather. They are unacquainted with ſpirituous liquors and tobacco; a ſure proof, that, hitherto, they have had no communication with the Europeans.

They reckon it an extraordinary ornament, to bore, in ſeveral places, the lower parts of the cheeks, near the mouth; and in [the holes] they ſet ſome ſtones and bones. Some wear, at their noſtrils, ſlate pencils, about four inches long; ſome wear a bone of that bigneſs, under the lower lip; and others a like bone on the forehead.

The nation, that lives in the iſlands round about cape Tchukotſki, and frequents the Tchutchi, is, certainly, of the ſame origin with thoſe people: for with them alſo it is thought an ornament [thus] to inlay [ingraft] bones.

Major Paulutſkoi, deceaſed, after a battle, which he once fought againſt the Tchutchi, found, among the dead bodies of the Tchutchi, two men of that nation, each of whom had two teeth of a ſea-horſe under the noſe, ſet in holes made on purpoſe: for which reaſon, the inhabitants of that country call them *Zubatui* [toothed]. As the priſoners reported, theſe [men] did not come to the aſſiſtance of the Tchutchi, but to ſee how they [uſed to] fight with the Ruſſians.

From this, it may be inferred, that the Tchutchi converſe with them, either in the ſame language, or, at leaſt, in languages of ſo great affinity, that they

can underſtand one another, without an interpreter; conſequently, their language has no ſmall reſemblance with that of the Koriaki: for the Tchukotchian comes from the Koriatſkian [language], and differs from it only in the dialect: nevertheleſs, the Koriatſkian interpreters can ſpeak with them, without any ſort of difficulty. With regard to what Mr. Steller writes, that not one of our interpreters could underſtand the American language, poſſibly, that comes from the great difference in the dialect, or from a difference of pronounciation; which is obſerved, not only among the wild inhabitants of Kamtchatka, but alſo among the European nations, in different provinces. In Kamtchatka, there is hardly any ſmall * *oſtrog*, but what the ſpeech there differs [ſomewhat] from that of another that lies neareſt. As for thoſe ſmall *oſtrogs*, which are at ſome hundreds of verſtes from one another, thoſe can no longer underſtand each other, without [ſome] trouble.

The following remarkable reſemblances between the American and the Kamtchadalian nations, have been obſerved:

1. That the Americans reſemble the Kamtchadales in the face.

2. That they eat the ſweet-herb, after the ſame manner as the Kamtchadales; [a thing] which never was obſerved any-where elſe.

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* *Oſtrojka*, a ſmall *oſtrog*, is a place fenced and fortified with a palliſade, made of trees, fixed perpendicularly in the ground, and cut ſharp at the top: ſometimes they are beams laid over each other. *Oſtrui*, in Ruſs, ſignifies ſharp.

3. That

3. That they make uſe of a * wooden machine [inſtead of a tinder-box], to light fire with.

4. That, from many tokens, it is conjectured, that they uſe axes made of ſtones, or of bones: and it is not without foundation, Mr. Steller thinks, that the Americans had once a communication with the people of Kamtchatka.

5. That their cloaths and their hats do not differ from the Kamtchadalian.

6. That they dye the ſkins with alder, after the Kamtchatka manner.

Which marks ſhew it to be very poſſible, that they came from the ſame race. This very thing, he rightly judges, may help alſo to ſolve that queſtion, "Whence came the inhabitants of America?" For though we ſhould ſuppoſe, that America and Aſia were never joined; nevertheleſs, conſidering the nearneſs of thoſe two parts of the world at the north, no one can ſay, that it was impracticable for people from Aſia to go over to ſettle in America; eſpecially, as there are iſlands enough, and at ſo ſmall a diſtance, which might facilitate not a little ſuch a paſſage [in order to ſettle].

Their armour for war is a bow and arrows. What kind of a bow it is, we cannot ſay, as it did not happen [to our people] to ſee any; but their arrows are much longer than the Kamtchadalian, and greatly reſemble the Tunguſian and Tartarian [arrows]. Thoſe, which came in the way of our people, were dyed black, and planed ſo ſmooth, that

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* See Deſcription of Kamtchatka.

they left no room to doubt of [the Americans] having alſo iron tools.

The Americans ſail upon the ſea in canoes made of ſkins, in the ſame manner as the Koriaki and the Tchutchi. * Their canoes are about two fathoms [14 feet] long, and about two feet high. The fore-part of them is ſharp; and they are flat-bottomed. Their inward frame conſiſts of ſticks, which are linked together at both ends, and in the middle are preſſed outwards, in a rounding [a belly], with croſs-ſticks [which keep the ſides at a proper diſtance]. The ſkins, which they are covered with, all around, ſeem to be thoſe of ſea-dogs, dyed of a cherry colour. The place where the Americans ſit is round, about two arſhines [4 feet 8 inches] from the poop; there is ſowed upon it the ſtomach [of ſome great fiſh], which one may gather and looſen as a purſe, with the help of thongs of leather, paſſed through ſmall holes at the edge. An American, ſitting in that place, ſtretches his legs, and gathers round him the ſtomach [above-mentioned], that water may not fall into the canoe. With one oar, ſome fathoms long, they row on both ſides alternately, with ſuch a progreſſive force, that contrary winds are but a ſmall hindrance to them; and with ſo much ſafety, that they are not afraid to go upon the water, even whilſt the ſea riſes in terrible ſurges. On the contrary, they look with ſome terror upon our larger veſſels, when they are toſſed, and adviſe thoſe, who ſail in them, to beware, leſt their veſſels ſhould be overſet. This

* See the two figures, in 1ſt vol. of the Deſcription of Kamtchatka, over againſt page 128.

happened

happened to the boat Gabriel, which, ſome years ſince, was going to cape Tchukotſki. As to the reſt, their canoes are ſo light, that they carry them with one hand.

When the Americans ſee [upon their coaſts] people, whom they do not know, they row towards them, and then make a long ſpeech: but it is not certain, whether this is by way of a ſpell [or charm], or ſome [particular] ceremony uſed at the reception of ſtrangers: for both the one and the other are in uſe among the Kurilians. But before they draw near, they paint their cheeks with black lead, and ſtuff their noſtrils with [ſome] herb.

When they have gueſts, they appear friendly; they like to converſe with them, and that in an amicable manner, without taking off their eyes from them: they treat them with great ſubmiſſion, and preſent them with the fat of whales, and with black lead, with which they uſed to beſmear their cheeks, as was before obſerved; doubtleſs from a notion, that ſuch things are as agreeable to others, as they are to themſelves.

With regard to the navigation about thoſe parts, it is ſafe [enough] in ſpring and in ſummer; but in autumn it is ſo dangerous, that hardly a day paſſes, but one has reaſon to fear being ſhipwrecked: for they [the Ruſſians ſent upon the ſea expedition] experienced ſuch a violence of winds and ſtorms, that even perſons, who had ſerved forty years at ſea, aſſured, with an oath, that they never ſaw ſuch in their lives.

The marks, by which they obſerve there [in that ſea] that land is near, are, particularly, the following

conſider-

conſiderable ones: 1. When there appears a great quantity of different kinds of the [ſo called] ſea-cabbage, ſwimming upon the water. 2. When one ſees the herb, of which, at Kamtchatka, they platt cloaks, matts, and bags; for it grows only on the ſea-ſhores. 3. When there begin to appear, at ſea, flights of ſea-mews, as well as droves of ſea animals, ſuch as ſea-dogs, and the like: for though ſea-dogs have a hole open at the heart, which is called *foramen ovale*, and a duct called *ductus arterioſus Botalli*, and, upon that account, may remain long under water, and, conſequently, go to ſome diſtance from the ſhore, without danger, inaſmuch as they can, at a greater depth, find food proper for them: nevertheleſs, it has been obſerved, that they ſeldom go farther than ten [German] miles from the ſhore.

The ſureſt ſign, that land is near, is, when there are ſeen Kamtchatka beavers, which live only upon crabs, and, from the make of their heart, cannot be under water above two minutes [at a time]; conſequently, they cannot get food at the depth of 100 fathoms, or indeed at a much leſſer depth; upon which account, they alſo breed always near the ſhore.

It remains ſtill to ſpeak of ſome iſlands neareſt to Kamtchatka, which are not found in a ſtrait line with the above-mentioned, but north of them; eſpecially of Berings iſland, which now is ſo well known to the inhabitants of Kamtchatka, that many of them go thither, to catch ſea-beavers, and the like ſea animals.

That iſland extends from the ſouth-eaſt to the north-weſt, between 55 and 60°. Its north-eaſt end, which lies almoſt directly over-againſt the mouth of the

the river Kamtchatka, is at about two degrees diſtance from the eaſtern ſhore of Kamtchatka: and its ſouth-eaſt end is about three degrees from cape Kronotſki. This iſland is 165 verſtes long; but its breadth is unequal. From the ſouth-eaſt end to a rock, which hangs perpendicularly over the ſea, and is at 14 verſtes diſtance from that end, the breadth of the iſland is from 3 to 4 verſtes: from that ſteep rock to Suiputchei bay, it is 5 verſtes: from Suiputchei bay to Beaver's ſteep rock, it is 6 verſtes: at Whale's-ſtream, it is 5 verſtes; but from thence farther on, it grows gradually broader. Its greateſt breadth [*viz.*], 23 verſtes, is over-againſt the north cape, which lies 115 verſtes from the above-mentioned end.

In general, it may be ſaid, that the length of that iſland is ſo diſproportioned to its breadth, that our author doubts, whether there can be, in other parts of the world, any iſlands of ſuch a ſhape; at leaſt, he never heard nor read of any ſuch: and he adds, that the iſlands, which they ſaw about America, and all the rows of them, lying to the eaſt, have the like proportion.

This iſland conſiſts of a ridge of rocks, which is divided by many vallies, that ſtretch to the north and to the ſouth. Its mountains are ſo high, that, in clear weather, one may ſee them from about half the diſtance between the iſland and Kamtchatka. The inhabitants of Kamtchatka, of old times, thought, that there muſt be [ſome] land over-againſt the mouth of the river Kamtchatka, by reaſon that [the ſky] appeared there always cloudy, though it were never ſo clear every-where elſe about the horizon.

The

The higheſt mountains [of the iſland] do not meaſure above 2 verſtes in a perpendicular.

[Here follows a deſcription of this barren rocky iſland, of which the following four pages are not to our preſent purpoſe. Then, at page 136, the account proceeds thus:]

The ſouth-weſt ſide of the iſland is of a quite different nature [from the other, as to acceſs]: for tho' the ſhore [there] is more rocky and craggy, yet there are two places, by which, in flat-bottomed boats, ſuch as are the * *tſcherbotui*, one may not only land on the ſhore, but even advance as far as a lake, by the ſtreams [that flow from it]. The firſt of theſe places is at 50 verſtes, and the other at 115, from the ſouth-eaſt end of the iſland.

This laſt place is very remarkable from the ſea; for the land there goes rounding from the north to the weſt; and, at the very promontory, there runs a ſtream, which is the largeſt of any in that iſland; and, when the water is high, its depth is not leſs than 7 feet. It runs from a great lake, which lies a verſte and a half from its mouth: and becauſe that ſtream grows deeper, the farther it is from the ſea, therefore one may conveniently go upon it in boats as far as the lake: and upon the lake there is a ſafe ſtation; for it is ſurrounded with rocky mountains, as with a wall, and ſheltered from all winds. The chief mark, by which one may know this ſtream from the ſea, is an iſland, which is about 7 verſtes in circumference, and lies to the ſouth at 7 verſtes diſtance from the mouth of the ſtream. The ſhore

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* Large canoes, or boats, ſomewhat reſembling ferry-boats.

from

from thence towards the weſt, is ſandy and low for 5 verſtes. Round the ſhores, there are no rocks under water; which one may know from thence, becauſe there are no * eddies [breakers].

From the higheſt rocks of that iſland, one ſees the following lands: at the ſouth, two iſlands, one of which meaſures about 7 verſtes in circumference, as was obſerved before; but the other is over-againſt the very end of Berings iſland, at the ſouth-weſt: it conſiſts of two high and cleft rocks, of about three verſtes in circumference, and is at 14 verſtes diſtance from Berings iſland.

From the north-eaſt end of Berings iſland, in clear weather, one may ſee, to the north-eaſt, very high mountains, covered with ſnow, and their diſtance may be computed at 100 or 140 verſtes. Thoſe mountains our author thought, with better grounds, to be a cape of the continent of America, than an iſland: 1. Becauſe thoſe mountains [allowing for] conſidering their diſtance, were higher than the mountains on the [neighbouring] iſlands. 2. Becauſe that, at a like diſtance towards the eaſt, one obſerves plainly, from the iſland, ſuch like white mountains, from the height and extent of which all judged that it was the continent.

From the ſouth-eaſt end of Berings iſland, they ſaw, to the ſouth-eaſt alſo, another iſland, but not very clearly: it ſeemed to lie between Berings iſland and [ſome] low part of the continent.

From the weſt and the ſouth-weſt ſides, it was obſerved, that, even in clear weather, there is a per-

* In German, *brennung*.

petual fog above [higher up than] the mouth of the river Kamtchatka; and from thence, in ſome meaſure, they came to know the inconſiderable diſtance of the land of Kamtchatka from Berings iſland.

North of the ſo often mentioned Berings iſland, there is another iſland, in length from 80 to 100 verſtes, which lies parallel to it, *i. e.* from the ſouth-eaſt to the north-weſt. The ſtreights between theſe two iſlands, at the north-weſt, meaſure 20 verſtes, and at the ſouth-eaſt about 40. The mountains upon it are lower than the ridge [of mountains] in Berings iſland. At both ends of it, there are, in the ſea, many rocks at [low] water mark, and perpendicular rocks like pillars.

With regard to the weather, it differs from that at Kamtchatka only in this, that it is more ſevere and ſharp: for the iſland has no ſhelter from any quarter; and, beſides that, it is narrow, and without woods.

Moreover, the force of the winds increaſes to ſuch a degree, in thoſe deep and narrow vallies, that one can ſcarce ſtand upon one's legs. In February and April months, were obſerved the ſharpeſt winds, which blew from the ſouth-eaſt and from the north-weſt. In the former caſe, the weather was clear, but tolerable; but in the latter caſe, it was clear, indeed, but extremely cold.

The higheſt riſing of the water happened in the beginning of February month, during north-weſt winds: the other inundation was in the middle of May, occaſioned by great rains, and by the ſudden thawing of the ſnows. Nevertheleſs, thoſe floods were moderate, in compariſon with thoſe, of which there [ſtill] remained undoubted marks: for there have

have been carried many trees, and whole ſkeletons of ſea animals, to the height of 30 fathoms, or more, above the ſurface of the ſea [above the common water mark, or level]: from which our author judges, that, in the year 1737, there happened, likewiſe, here [in this iſland] ſuch an inundation as that at Kamtchatka.

Earthquakes happen [here] ſeveral times in the year. The moſt violent, that was obſerved, was in the beginning of February, which, during a weſterly wind, laſted exactly ſix minutes; and before it was heard a noiſe, and a ſtrong wind, under ground, with a hiſſing, which went from ſouth to north.

Among mineral things, which are found in that iſland, one may reckon, as the moſt remarkable, the fine waters, which, upon account of their pureneſs and lightneſs, are very wholſome: and this virtue of them was obſerved upon ſick people, with advantage, and the deſired ſatisfaction. With regard to the plentifulneſs of them, there is not a valley but what has a ſtream running through it; and the number of them all together exceeds ſixty; among which there are ſome, which are from 8 to 12 fathoms broad; and ſome are two, and ſome even 5 fathoms deep, when the water is high; but there are few ſuch, and the greater part of them is extremely ſhallow at the mouth; becauſe that they have a very rapid courſe, on account of the ſteep ſlopes of the vallies, and that near the ſea they divide into many rivulets.

Explication of ſome Words, which occur in the Reiſe-Beſchreibung, *&c.*

Page

6. *Promyſchleni* properly ſignifies thoſe, that make a trade or profeſſion of hunting in Siberia.

7. *Goſtinna ſotna*, the merchants of the firſt claſs in Ruſſia.

Ibid. *Semeon Iwanow ſin Deſchnew.* When the Ruſſians ſpeak of a man of quality, they add to his Chriſtian name that of his father; *ex. gr. Simon Ivanowitſch Deſchneff*; but if of a common man, they ſay, as here, *Simon Ivanoff ſin* (*i. e.* ſon of) *Deſchneff*, Simon the ſon of John Deſchneff.

8. *Coſacke.* The Coſacks are properly the ſoldiers, of whom conſiſts the Ukrain militia, or that of the neighbourhood of the Don. Theſe laſt began the conqueſt of Siberia. Their poſterity is ſtill employed in raiſing the taxes laid on the Siberian nations, which have ſubmitted to the Ruſſian empire.

Ibid. *Zimmer*, is a bundle of furs, conſiſting of 40 ſkins.

11. *Simowie* [*zimovie*], properly, is an habitation, or quarters, for the winter; a hut or huts, built haſtily, in ſome deſert place, in order to ſpend a winter there. When the number of thoſe huts has ſo increaſed, as to become a village, it has often ſtill retained the old name of *zimovie*. *Zima*, in Ruſſian, is winter.

Oſtrog.

Page

11. *Oſtrog.* See page 484 above.

14. *Narte*, a kind of ſmall ſledge, very light, drawn by dogs. See the repreſentation of it in the Deſcription of Kamtchatka.

19. *Werchnei*, *niſchnei*, *ſerednie*, *bolſche*; theſe words ſignify, high, low, middle, and greater; as *werchnei Oſtrog*, the upper Oſtrog, *&c.*

24. *Swaetoi*, ſignifies holy, ſaint; as *Swaetoi noſs*, cape Holy.

26. *Piaetideſaetniſs*, an officer, who has 50 men under his command, and who may be called a lieutenant. In Ruſs, *pietderiat* is fifty.

27. *Woewood* is the governor of a town, of a diſtrict, or of a province.

33. *Stolnik* was an officer, formerly, who ſet the diſhes upon the tſar's table. *Stol*, a table.

Ibid. *Knjaes*, a prince.

34. *Dworaen*, a gentleman.

Ibid. *Uſt*, mouth. *Uſt Jana*, the name of a place ſituated at or near the mouth of the river Jana.

Ibid. *Noſs*, cape, promontory, neſs, neeſe. The The proper ſignification of the word is noſe.

40. *Schikiti*, ſewed, from *ſchit*, to ſew.

41. *Werſte*, an itinerary meaſure in Ruſſia, conſiſting of 500 *ſajens*, or fathoms, each of which makes 7 feet Engliſh.

Sin

Page

42. *Sin bojarſkoi*, a title which is conferred, in Siberia, upon low people, in order to raiſe them to a degree nearer to that of a noble or a *gentilhomme*.

59. *Vielfraſs*, a German word, which ſignifies a glutton. It is a wild beaſt of a ſmall ſize, about the bigneſs of a fox, but more reſembling a bear. This creature, called in French, *glouton*, is extremely voracious, from whence it had its name.

Ibid. *Steinfuchs*, is a kind of fox.

86. *Baidar*, canoe.

Ibid. *Neſſel*, a nettle, in general, in German; here a kind of nettles, of which a particular ſtuff is made.

Ibid. linea penultima. *Levkaſhenuiu poſudu*, ſignifies a veſſel or pot made of *levkas*, a kind of alabaſter. Here it probably was intended to expreſs China ware, or porcelaine.

90. *Rietgraſs*, a German word, ſignifying the ſame as bamboo.

121. *Judomſkoi kreſt*, Judoma's-croſs, the name of a village near the river Judoma, in a place on which was erected a croſs, when it was firſt diſcovered.

Ibid. *Belſkoi perewoſs*, the ferry of Bela.

Ibid. linea penult. *Jiujnoi*, ſoutherly, to the ſouth.

143. *Krepoſt*, ſignifies a caſtle, or a fort.

Ibid. *Sawod*, a manufactory, or fabrique.

Ibid. *Woſkreſenſkei*, belonging to the reſurrection.

Plot-

Page

165. *Plotbiſchtſche*, the place where are built the *(plotui)* floats, or ſmall boats.

181. *Buxiren*, a German word, which ſignifies to tow, to take in tow.

182. *Dannen* (or *tannen*) *knoſpe*, the buds of fir-trees.

151. *Buikowſkoi muis*, *parvum promontorium bovinum*.

251. *linea antep.* } Read *korova morſkaia*, the ſea-cow.

250. *lin.* 6. } Read *viporotki*.

249. *lin.* 16. } Read *kotui morſkie*, ſea-cats.

Guba, a gulf, or bay.

Matroſs, a ſailor.

Nova Zemla, New Land, *Terra Nova*.

Gorodock, a ſmall town, *oppidulum*.

Sloboda. See the introduction to the *Atlas Ruſſicus*, in which many Ruſſian names of places, *&c.* are explained.

XLVII. *Remarks on the Mutations of the Stars*; *by* Tho. Barker, *Eſq*; *of* Lyndon, *in* Rutland: *Communicated by the Rev.* W. Stukeley, *M. D. F. R. S.*

Read Jan. 31, 1760. IT is well known there have been ſeveral alterations among the fixed ſtars: for inſtance, Ptolemy's *ultima fluvii*, a firſt magnitude ſtar, is in Dr. Halley's catalogue of the ſouthern conſtellations only a third magnitude: and in much leſs time, the δ of the Great Bear, which Bayer ſeems to have judged juſt of the ſame ſize with the other ſix, is grown far duller than any of them. Some ſtars alſo have quite diſappeared, while again new ones, not ſeen before, have been diſcovered: and there are others periodically larger and ſmaller. Two very remarkably bright, yet ſhort-lived, ſtars, have been alſo ſeen, one in Caſſiopeia, the other in Serpentarius; which breaking out, at once, with greater luſtre than any other fixed ſtar, gradually faded, and changing to different colours, in about a year and half were no longer viſible. But, I think, no one has yet remarked, that any laſting ſtar was of a different colour in different ages: Greaves, on the contrary, takes notice, that the colours of the ſtars and planets are the ſame now as the antients obſerved; which is, I believe, very true in general: for Ptolemy, in his catalogue of ſtars, ſays, Arcturus, Aldebaran, Pollux, Cor Scorpii, and Orion's Shoulder (with another to be mentioned preſently), are ὑποχιππος, reddiſh: and the five here mentioned are ſtill of

of that colour, and, I think, the only conſiderable ſtars which are ſo.

But, to this rule there ſeems to be one exception, and that in a remarkable ſtar: for old authors mention the Dog ſtar, which is now white, and not at all inclined to redneſs, as being then very much ſo; as in the following places:

Τοιος και φρϑρος αειρομενω ὑπο νωτω
Φαινεται αμφοτεροισι κυων ὑπο ποσσι βεβηκως
Ποικιλος——— Αρατϑ φαινομενα. 326.

Which Cicero thus turns——See Gruter's Cicero, IV. 359.

Namque pedes ſubter rutilo cum lumine claret
Fervidus ille Canis ſtellarum luce refulgens.
Seu rubra Canicula findet
Infantes ſtatuas—— *Hor. Sat.* II. 5, 39.

Acrior ſit Caniculæ rubor, Martis remiſſior Jovis nullus. *Seneca Quæſt. Nat.* I. 1.

Ὁ εν τω ϛοματι λαμπροτατος καλϑμενος κυων ὑποχιρρος. *Ptolemy.* Κυνος αϛερισμος.

Ποικιλος, in the quotation from Aratus, does not expreſly mean red; but is always uſed of ſomething ſhewy, glittering as gold, various-coloured, *&c.* as in the following places.

Τειχεα ποικελα χαλκω. *Homer's Il. v.* 181.
Παρδαλεη μεν πρωτα μεταφρενον· ευρι καλυψε
Ποικιλη——— Ιλ. κ. 30.
Ὁς καλλιϛος εην ποικιλμασιν—— Οδ. ο. 107.
Βητην εις Οδυσηα δαιφρονα ποικιλομητην. Οδ. χ. 202.

Aratus therefore, I think, ſhews at leaſt, that the Dog ſtar was not then of the ſame colour as other ſtars: and, as Cicero turns it *rutilus*, it appears he either underſtood the word to mean red, or knowing by his own view it was ſo, thought it the proper interpretation; for *rutilus* is uſed of what is reddiſh, and often of the red glare of a fire, or the dawn, as below:

——rutilum vomit ille cruorem. *Ovid. Met.* V. 33.

Promiſſæ et rutilatæ comæ. *Livy.* XXXVIII. 17.

Arma inter nubem, cœli in regione ſerena.
Virg. Æneid. VIII. 528.

Per ſudum rutilare vident,
Sin maculæ incipient rutilo immiſcerier igni.
Georg. I. 454.

Auroram rutilare procul cerno.
Varro, de Ling. Lat. VI. 5.

Rubra, in Horace, will, I think, bear no other ſenſe than red, or elſe it is the heat he there chiefly ſpeaks of: and though, I think, Latin authors confound *Canicula*, ſome uſing the word for *Sirius*, others for Procyon; yet it plainly appears, that *Sirius* is here meant, ſince Horace always calls it *Canicula*, and never uſes the word *Sirius*: but Aratus and Ptolemy leave no room to doubt what ſtar it was, being expreſly ſpeaking about the Dog ſtar.

Seneca ſays, the redneſs was ſo ſtrong as to exceed that of Mars, to which no ſtar now approaches. None of the notes on Seneca clear up this matter: Fromondus, indeed, obſerved the place, and declared his aſtoniſhment at it; but does not attempt to

to ſolve the difficulty. Ptolemy's is, however, the moſt undeniable evidence, who, when directly deſcribing the ſtars, and particularly mentioning the Dog ſtar, ſays, expreſly, it was of the ſame colour as Cor Scorpii, and the other ſtars, which are ſtill red; ſo that I do not ſee how his evidence can be diſputed.

There is, however, one objection to what I have ſaid, but I, think, not an unanſwerable one; which is, that, at firſt ſight, Hyginus ſeems to call Sirius white: but ſince, if ſo, he contradicts the other authors I have above quoted to prove it red, and, becauſe he there ſays ſomething I do not well underſtand, I ſhall quote the whole.

Hygini Poetic Aſtron. II. 35.

Canis habet in lingua ſtellam unam, quæ ipſa Canis appellatur; in capite autem alteram, quam Iſis ſuo nomine ſtatuiſſe exiſtimatur, et Sirion appellaſſe propter flammæ candorem; quod ejuſmodi ſit, ut præter cæteras lucere videatur, itaque quo magis eam cognoſcerent, Sirion appellaſſe.

He again diſtinguiſhes theſe two ſtars, *lib.* III. 34.

Canis habet in lingua ſtellam unam, quæ Canis appellatur, in capite autem alteram, quam nonnulli Sirion appellant, de quo prius diximus.

Of two ſtars in the Dog's head, Iſis and Sirius, Eratoſthenes alſo ſpeaks:

Καταστερισμος λγ. Κυων —— Εχει δε αστερας, επι μεν της κεφαλης ἀ ὁς Ισις λεγεται, της γλοττης ἀ ὸν και Σειρων καλεσι μεγας δε εστε ϗ λαμπρος, τες δε τοιετες

αςερας οι αςρολογοι σειριος καλεσι δια την της φλογοειδοτητος κινησιν.

Hyginus, in diſtinguiſhing Canis from Sirius as two different ſtars, ſeems, to me, to contradict all other writers, who ſpeak of them as one, except, perhaps, two or three latter ones, who directly quote Hyginus's words. Sirius, or Canis, the brighteſt ſtar in the heavens, is that, which Ptolemy calls in the mouth; Eratoſthenes and Hyginus, in the tongue: but whether Bayer γ, which Flamſtead calls a third magnitude ſtar, Ptolemy only a fourth, was in more antient times larger, I will not pretend to ſay; ſince, Eratoſthenes and Hyginus both ſpeak of two ſtars in the Dog's head, as thought worthy of particular names. If, in Hyginus, *flammæ candorem* means the whiteneſs of its light, as *candor* often does, he expreſly contradicts what I have quoted above from others; yet ſtill I think Ptolemy's authority is greater than that of Hyginus. But that *candor* is alſo uſed for innocence, beauty, brightneſs, *&c.* take the following examples.

Bis ſenis equis candore eximio trahentibus. *Suet. Cæſ. Octav.* 94.

Si tamen ille prior, quo me ſine crimine geſſi
Candor——— *Ovid. Epiſt.* IV. 31.

——— formæ niſi candor. *Metam.* I. 743.

Candore noto reddas judicium peto.
Phædrus. III. *Prol.* 64.

Pendebant ex auribus inſignes candore et magnitudine Lapilli. *Quint. Curt.* IX. 4.

Ut cum videmus ſpeciem primum, candoremque cœli. *Cic. Tuſc. Quæſt.* I. 28.

Solis candor illuſtrior quam ullus ignis. *De Nat. Deor.* II. 15.

In the ſecond or third laſt quotation, *candor* is uſed in the ſame ſenſe as in Hyginus, for brightneſs, without regard to colour; for ſo, I think, he muſt be underſtood, not only to avoid contradiction between him and Ptolemy, but from the name *Sirius*, which it could not be called from its whiteneſs, Σειριος bearing no relation to that, but to brightneſs, heat, or dryneſs; all which the antients ſpeak of, as properties of the Dog ſtar. Again, it is brightneſs, wherein it excells all other ſtars, and not in whiteneſs; for Orion's foot and others are as white, but there is none ſo bright as the Dog ſtar. All this is ſaid, on ſuppoſition there was but one remarkable ſtar in the Dog's head, that in the mouth: for if there were two, as Hyginus ſays, we are not here concerned with either the brightneſs or colour of his *Sirion*, which was in the head, as it certainly faded before Ptolemy's time, who mentions only one, that in the mouth, and which, he ſays, was then red, but is now white.

To conclude the whole; however remarkable and without precedent it may be, that ſo noted and laſting a ſtar as the Great Dog ſhould have changed its colour, yet as at leaſt five different writers affirm it, ſome ſo expreſly, and where their ſubject required them to ſpeak particularly about it, it appears to me to have been certainly the caſe. If, however, any one, ſtartled at the ſtrangeneſs of the thing, thinks the

the evidence I have brought inſufficient to prove it, he is deſired to invalidate what I have here ſaid, by a deduction of freſh evidence, and to account for theſe ſeveral expreſſions in old authors, which ſeem to prove, that ſuch a change has really happened.

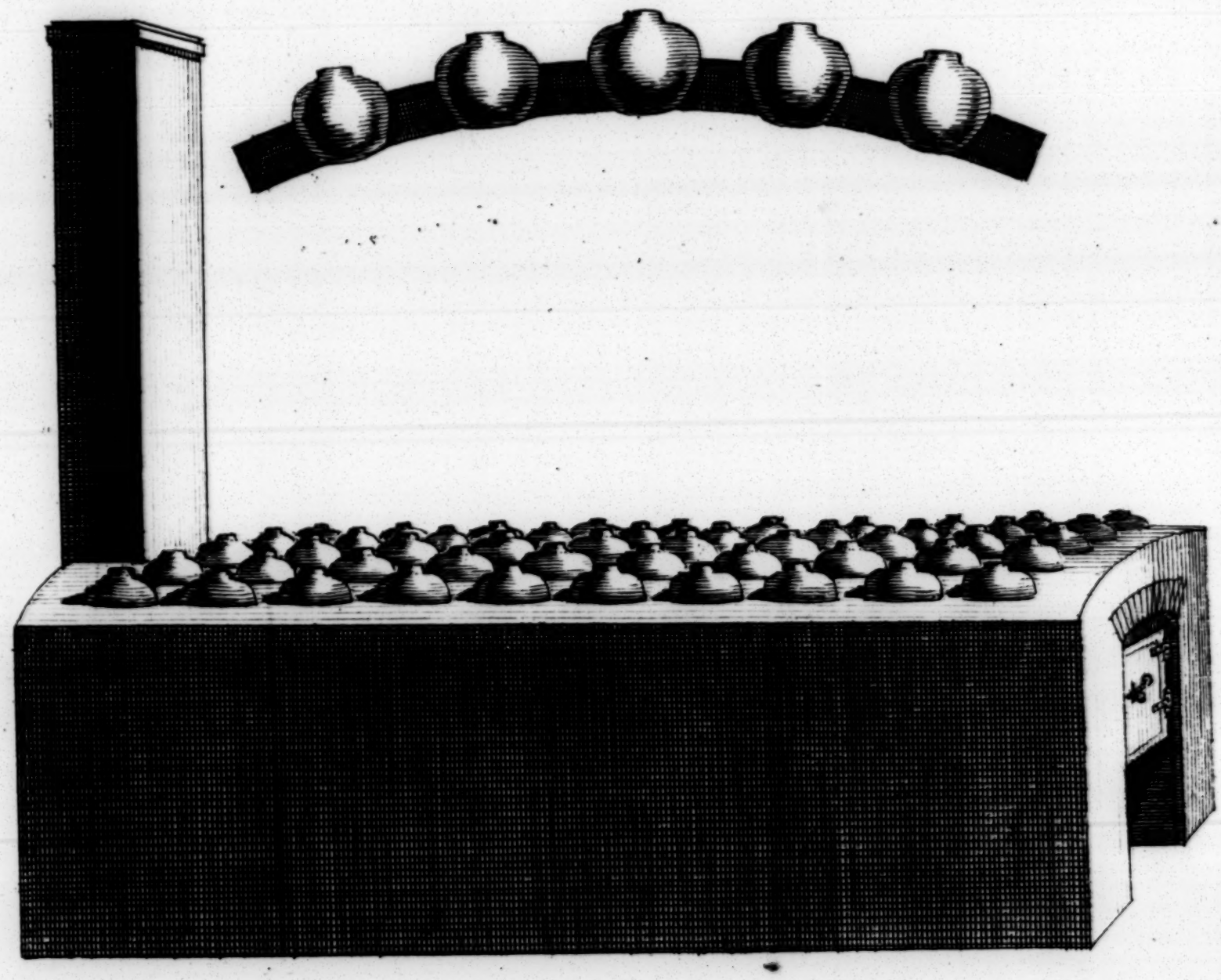
XLVIII. *The Method of making Sal Ammoniac in* Egypt; *as communicated by Dr.* Linnæus, *from his Pupil Dr.* Haſſelquiſt, *who had been lately in thoſe Parts: By* John Ellis, *Eſq*; *F. R. S.*

Read Jan. 31, 1760.

SAL Ammoniac is made from the ſoot ariſing from the burnt dung of four-footed animals, that feed only on vegetables.

This dung is collected in the four firſt months of the year, when all their cattle, ſuch as oxen, cows, buffaloes, camels, ſheep, goats, horſes, and aſſes, feed on freſh ſpring graſs, which, in Egypt, is a kind of trefoil, or clover: for when they are obliged to feed their cattle on hay, and their camels on bruiſed date kernels, their excrements are not fit for this purpoſe; but when they feed on graſs, the poor people of Egypt are very careful to collect the dung quite freſh, and, for that purpoſe, follow the cattle all day long, in order to collect it as it falls from them; and, if it is too moiſt, they mix it with chaff, ſtubble, ſhort ſtraw, or duſt, and make it up in the form of cakes, about the ſame ſize and ſhape as it lies on the ground.

Then



A Plan of the subliming Furnace or Oven for making Sal Ammoniac in Egypt taken from the description here given.

J. Mynde sc.

Then they fix it to a wall to dry, till it is fit to be burnt.

For want of wood, which none but the rich in Egypt can afford to buy, they burn this dung through the whole country, and ſell a vaſt quantity of it to the ſalt-makers.

The excrements of the camel are not found at all preferable to any other; and its urine is never uſed for this purpoſe, although generally reported ſo by authors.

The ſalt-workers pretend, that the human excrements, and thoſe of goats and ſheep, are preferable to any other.

The months of March and April is the only time they make the ſalt.

Sal ammoniac is made in the following manner:

They build an oblong oven, about as long again as broad, of brick and moiſt dung, of ſuch a ſize, that the outſide, or flat part of the top of the arch, may hold fifty glaſs veſſels, ten in length, and five in breadth, each veſſel having a cavity left for it in the brick-work of the arch. *See Tab.* XI.

Theſe glaſs veſſels are globular, with a neck an inch long, and two inches wide.

Theſe veſſels are of different ſizes, in different ſalt-works, containing from a gallon to two gallons; but, in general, are about 18 inches diameter.

They coat each veſſel over with a fine clay (which they find in the Nile), and afterwards with ſtraw; they then fill them two-thirds full of ſoot, and put them into their holes on the top of the oven.

They make the fire gentle at firſt, and uſe the afore-mentioned dried dung for the fuel; they in-

creaſe

creaſe the heat gradually, till they bring it to the higheſt degree, which the workmen call hell-fire, and continue it ſo for three days and three nights together.

When the heat is come to its due degree, the ſmoke ſhews itſelf, with a ſouriſh ſmell, that is not unpleaſant; and, in a little time, the ſalt ſticks to the glaſſes, and covers the whole opening. The ſalt continues ſubliming, till the above-mentioned time is expired; then they break the glaſſes, and take out the ſalt, juſt in the ſame form, and of the ſame ſubſtance, that it is ſent all over Europe.

At each ſalt-work, they have a glaſs furnace, to melt the old glaſſes, and make new ones.

XLIX. *Montium quorundam prælatorum, magna ligni foſſilis copia quaſi infarctorum, brevis deſcriptio* Sam. Chriſt. Hollmanni, *Philoſophiæ Profeſſor.* Goettingenſis, *et S. R. Sodalis.*

Read Feb. 14, 1760.

SUNT in Haſſiæ et principatus Goettingenſis confiniis montes quidam prælti, qui tanta *ligni foſſilis* copia abundant, ut fidem fere ſuperet. Horum alter non procul Mundenſi oppido, circa Werram Fuldamque confluentes poſito, verſus meridionalem plagam ad tantam altitudinem aſſurgit, ut mercurius barometricus in eodem ad $1'' 25'''$ pedis Lond. ab illo gradu deſcendat, quem ad Werræ Fuldæque, ipſiuſque adeo ex iiſdem oriundi Viſurgis, ripas habet: unde ex repetitis in utroque

utroque loco obſervationibus, mercuriique adeo altitudine media, utrobique, quam maxime fieri potuit, determinata, montis, de quo nunc pluribus dicendum eſt, altitudinem ad 1150 circiter pedes Pariſinos revocare licebit. In hujus montis ſummo fere vertice planities quædam, ſi ita appellare fas eſt, reperitur, quæ ad ſexcentarum circiter orgyarum latitudinem, ſeptingentarum vero longitudinem, excurrit, ſub cujus ſuperficie, ad 5, 10, 15 pedum profunditatem ingentia lignorum foſſilium ſtrata reperiuntur, quæ in illis locis, ubi ligna iſta foſſilia ad ſuos, quibus deſtinata erant, uſus eruebantur, 20 circiter pedum altitudinem tunc temporis, quando ante paucos annos ibidem aderam, habebant. Quoniam ſtratorum vero iſtorum fundus, cui inſiſtebant, lapideus, *die ſohla* metallifoſſores noſtri vocant, dimidium circiter pedem craſſus ſolum erat, Dominuſque fundi reſcire cupiebat, num infra fundum iſtum lapideum ejuſdem ligni copia aliqua forſan ſupereſſet; terebra quadam, quali ad exploranda montium viſcera uti metallifoſſores nonnunquam ſolent, periculum factum eſt, repertumque, infra fundum iſtum lapideam ejuſdem ligni copiam ad 30 circiter pedum profunditatem adhuc ſupereſſe, neque ad novum tamen ejuſdem fundum perventum adhuc erat; eandemque ligni iſtius foſſilis altitudinem in aliis etiam ejuſdem montis locis, ubi inquirere in eaſdem licuit, reperiri, mihi confirmatum eſt. Hæc ligna in ſtratis ſuis ita ſibi incumbunt, ut in ſectione ipſorum perpendiculari, qualis tunc temporis, quum ibidem adeſſem, ad fundum ipſorum lapideum uſque ad 20 circiter et plurimum pedum latitudinem facta erat, ligna ipſa lapidum, in lapicidinis ſibi incumbentium, ſitumque horizonti parallelum ſervan-

tium, formam fere referens, adeo arcte etiam ſibi invicem quaſi impacta ſint, ut vix terræ interlabentis quicquam obſervare liceat: unde difficillimum cognitu evadit, num *arbores integræ*, an *arborum* jam fiſſarum diviſarumque *partes* ſolum, fuerint, ex quibus iſta enata ſint. Temporis etiam, quo hunc ſitum procul dubio jam obtinuerunt, longinquitate, terræque ſimul incumbentis et aquæ interlabentis humiditate, adeo fragilia facta ſunt, ut non in naturali ſuo ſitu ſolum rimarum, verſus omnes fere directiones excurrentium, plena ſint, ſed loco etiam ſuo mota in majores facile minoreſve portiones dilabantur. Color omnium horum lignorum a fuſco ad nigrum fere tendit, quo etiam terra illa pauca paſſim intermixta non ſolum, ſed eidem etiam incumbens, ad pedis unius et ultra altitudinem, ſimul tincta eſt: unde et hac, licet caute remota, ne in extrema quidem ſuperficie juſtam lignorum iſtorum faciem, imo ne ſitus quidem directionem veram, cognoſcere ſatis diſtincte licet. Terra enim iſta humida ligni ſemiputridi rimæ et intervalla omnia adeo oblita repletaque ſunt, ut viſum omnem fallant confundantque. Quæ huic vero terræ incumbit, humus eſt intermixto ſabulo, in qua optimæ paſſim argillæ bolique, alibi vero arenæ etiam ſubtiliſſimæ, varieque coloratæ, reperire ſtrata licet. Ex his ipſis terrarum circumjacentium ſtratis vero novus quaſi adhuc aſſurgit in ſummo cacuminis vertice mons, quingentorum et quod excurrit paſſuum ambitum circiter habens, lapidibus ingentibus fere totus conſtratus, arboribuſque præterea proceris confitus, ſub quo ipſo quoque lignorum iſtorum ſtrata quædam adhuc excurrunt.

Ligna,

Ligna, quæ ex ſtratis hiſce eruuntur, limo humoque interlabente ita plerumque inquinata et obducta ſunt, ut a gleba quadam terreſtri primo adſpectu non multum differant: unde et a foſſoribus et operariis, qui iiſdem eruendis adhibentur, *carbones foſſiles* plerumque appellantur. Quando aqua vero leviter abluta et purgata ſunt, aerique ficciori per breve temporis ſpatium expoſita, naturam indolemque ſuam genuinam adeo clare ſtatim produnt, ut vel imperitiſſimus et ſtupidiſſimus quiſque ne per momentum quidem temporis dubitare poſſit, num ligna vere talia ſint, an minus. Quod nudo vero oculo in iiſdem ita jam agnoſcitur, illud ipſum vel leviter armato magis adhuc pateſcit: ſiquidem et *tubuli* lignorum *perpendiculares*, cum *fibris* ipſorum *tranſverſis* et *horizontalibus*, facillime in iiſdem dignoſcuntur, et *incrementa* ipſorum *annua* clariſſime in iiſdem apparent; ut alia ejuſmodi plura taceamus. Quando fruſtra etiam illa lignea in juſtæ magnitudinis acervos convehuntur, hique eadem illa enchireſi tractantur, qua carbonarii noſtri in parandis ex ligno recenti carbonibus utuntur, optimæ notæ *carbones lignei* inde prodeunt; qui ab acido vero, quod magna copia in iiſdem adhuc reſidet, minerali non eoſdem ubivis cum aliis carbonibus ligneis uſus præſtant. Ligna enim hæcce foſſila, quorum formam ſitumque hactenus deſcripſi, tanta *bituminis aluminiſque* copia impræg-nata ſunt, ut non ignem ſolum facile concipiant, alantque, ſed et cineres, qui ab illorum ſub diu facta combuſtione ſuperſunt, magnam aluminis copiam fundant: quod ex iiſdem etiam in peculiari huic uſui deſtinata officina, magna copia, in eodem montis vertice adhuc excoquitur. Atque hæc cauſa etiam

eſſe videtur, cur elegantiſſimi inter ligna iſta non raro reperiantur *pyritæ*, qui aliorum ſimilium more ſcintillas ex chalybe copioſſimas eliciant; aeri vero liberiori per aliquod tempus expoſiti, primum dehiſcere, mox in pulverem reſolvi, toti quanti ſoleant, qui aluminis ſulphuriſque vulgaris floribus quaſi, paucaque terra martiali, vel nudo oculo conſtare obſervatur. Quoniam inter eoſdem vero nonnunquam reperiuntur, qui arborum fruticumque ramentis circumfuſi formas elegantiſſimas referunt, quos ab interitu ſuo, ſi fieri poſſet, conſervare cupiebam, aquam ferventem aliquoties ſingulis diebus ipſis tamdiu affudi, donec odore et colore fere omni carens ab iiſdem rediret: atque hac equidem ratione factum eſt, ut illorum nonnulli, probe prius exſiccati, atque chartis thecisque ſiccis involuti, et ab aeris humidioris acceſſu defenſi, jam ſex fere annos perdurent, parumque mutationis adhuc paſſi ſint. Horum nonnulli media fruſti cujuſdam lignei ſubſtantia ita ſæpe occultantur, ut non niſi illo fracto, fiſſoque, in conſpectum prodeant.

Alter horum montium, de quibus dicere conſtitum eſt, in ipſa Haſſia, prope Allendorffium, quod a fontibus et officinis ſuis ſalaribus longe lateque his in regionibus celebratur, ſitus, atque ab accolis propterea procul dubio *der Wyſzner*, mons albus, appellatus eſt, quod cacumen ipſius diu adhuc ab incumbente nive albeſcit, quando in reliquorum montium circumjacentium verticibus dudum illa diſparuit. Hic mons priorem iſtum altitudine multum adhuc vincit, atque ex obſervationibus barometricis, in eodem etiam aliquoties repetitis, ſupra Werræ, ipſum quoque proxime præterlabentis, ripam ad 1970 circiter

citer pedum Pariſinorum altitudinem quam proxime aſcendit. In montis hujus viſcera, circa 1500 circiter pedum inde a baſi ipſius altitudinem, ab orientali, meridionali, et occidentali ejuſdem plaga *cuniculi*, quos *mellen* Germani noſtri vocant, per duriſſimum partim lapidem ducti ſunt, quos magna bituminis foſſilis lignorumque foſſilium copia undique ambit. Horum unum, ſubſtructionibus ſuis munitum, ſumto metallifoſſoris habitu, ad *quingentarum* circiter *orgyarum*, quarum ſingulæ hexapedam Gallicam paulum adhuc ſuperant, longitudinem, finemque adeo ipſum illius cuniculi, ſubii, nihil ab omni latere, tectoque incumbente, quam *bituminum lignorumque foſſilium* immenſam copiam, poſt ſubſtructiones illas ligneas, ad lampadum flammam cum comitibus meis videns. Atque bitumen quidem, quod hic reperitur, et eruitur, nigredine et ſplendore ſuo ad gagatem ipſum quam proxime accedit, duritie vero parum per ab eodem differt. Lignum vero foſſile ejuſdem cum altero illo, quod modo deſcripſi, et a cujus antea indicata ſede hic mons etiam paucis milliaribus Germanicis ſolum diſtat, videtur eſſe generis; aluminis vero, aut alius ſalis mineralis, bituminiſque, quantum adhuc conſtat, parum admodum continet, coloris etiam minus fuſci, et nigricantis eſt. Per hoc lignum cuniculus ille, quem ſubii, ita ductus erat, ut hunc ipſum undique ambiret, cuniculique ipſius ſubſtructionibus in ſitu ſuo naturali conſervaretur, ejuſque altitudinem ab utroque cuniculi latere 4 aut 5 orgyarum plerumque eſſe foſſores confirmabant. Hiſce lignis *bituminis* illius, quod modo indicavi, ſtratum, ad *duarum* circiter *orgyarum* altitudinem, adeo exacte ubivis incumbit, ut ſuperfuſum quaſi eidem eſſe videatur:

deatur: unde et proxime eidem ſubjacentium ſuperiorum lignorum fruſtra multum eodem imprægnata ſunt, propiuſve ad colorem ipſius nigricantem accedunt. Quum hujus igitur bituminis tanta intra montis hujus viſcera reperiatur copia, ut a ducentis fere annis jam ad excoquenda ſalia Allendorffienſia ſit adhibitum, illoque ipſo, quum ibidem adeſſem, tempore vel ſingulis annis ultra ducenties mille modios, menſuræ Goettingenſis, ex eodem adhuc erui, earundemque ſalinarum uſibus impendi, dicerentur; ligna iſta foſſilia, quæ eodem bitumine imbuta non ſunt, intacta fere a foſſibus relinquuntur, futurorum ſæculorum uſibus demum inſervitura. Quemadmodum hæc vero bitumina lignis ſubjacentibus foſſilibus, ita bituminis hujus ſuperficiei *tectum*, quod ipſum proxime excipit, *lapideum* adeo arcte ubivis incumbit et cohæret, ac ſi vel ipſum infuſum eidem eſſet, adeo ut lapidum bituminumque ſubſtantiæ ſibi invicem quaſi permixtæ ibidem multis in locis appareant. Quanta tecti hujus incumbentis lapidei craſſities ſit, non ſatis equidem conſtat, neque in omnibus forſan locis eſt eadem. A tecti vero hujus cum ſubjacentibus bituminibus confinio ad ſummum montis, ad quod per acclivitatem ejus paſſim aſcenditur, cacumen, *quingentorum* circiter *pedum* Pariſinorum altitudo adhuc multis in locis eſt: hocque montis cacumen procerarum arborum ſylvis paſſim tegitur, ſuperius vero, ubi in ampliſſimam tandem planitiem obit, prato quodam ſatis ſpatioſo et udoſo ſimul inſternitur. Ligna ergo hæcce, cum incumbente bituminum ſtrato ſatis craſſo, *mediis* fere montis ingentis *viſceribus* incluſa ſunt; quum priorum iſtorum magna pars, non admodum profunde, pleriſque in locis, humo

cooperta

cooperta ſit, parſque ſolum illorum aliqua ſub accumbentis ſupremi montis verticem, quantum adhuc conſtat, excurrat. Utrumque tamen tantum naturæ eſt miraculum, ut ſatis vivis depingi coloribus vix queat, neque parum ad illuſtrandam globi noſtri terraquei hiſtoriam conferre vel maxime aliquando poſſit.

Ejuſdem forſan generis eſt, quod ex *Societatis Reg. Goettingenſis Commentariorum tom.* II. paucis hic repetere liceat. Reperta nempe ante aliquot annos, et ex marga eruta, in montium noſtrorum Hercynicorum vicinia, ſunt oſſa quædam ingentis, et fere monſtroſæ, magnitudinis, quæ Elephantis cujuſdam reliquiæ primo intuitu eſſe fere omnibus videbantur. Paulo accuratiore vero illorum inſtituto examine, facile apparebat, oſſa iſta animalis cujuſdam fragmenta eſſe, minoris quidem elephante altitudinis, ejuſdem tamen, ſi non majoris, cum eodem molis et ponderis. Quum eodem vero in loco paulo poſt etiam *dentes* eruerentur, inuſitatæ magis formæ, quam magnitudinis, qui vivi iſtius, per nonnullas Germaniæ Galliæque provincias tunc temporis peregrinatis, rhinocerotis dentibus exacte ſimiles reperiebantur eſſe; *rhinocerotis* oſſa iſta foſſilia eſſe, eo minus relinquebatur dubii, quod nullius adhuc cogniti, ſive terreſtris, ſive aquatilis, animalis ſimilitudinem iſta ullam referrent. Tot vero eorum oſſium eodem in loco reperta ſunt fragmenta, ex quibus clariſſime patet, *quatuor* minimum *adulta*, unumque *minus adultum*, ejuſdem generis individua eodem in loco ſepulta fuiſſe: id quod occaſionem mihi poſtea dedit, viam quandam novam tentandi, qua corporum hujuſmodi peregrinorum, ipſorumque marinorum, in terræ continentis diverſis regionibus origo intelligi forſan melius poſſet: de quo

in

in iiſdem Societatis noſtræ *Commentariorum tom.* IV. jam fuſius dictum eſt.

L. *Experiments in Electricity: In a Letter from Father* Beccaria, *Profeſſor of Experimental Philoſophy at* Turin, *to* Benjamin Franklin, *L. L. D. F. R. S.*

Benjamino Franklin viro de re electrica meritiſſimo Joannes Baptiſta Beccaria ex ſcholis piis S.P.D.

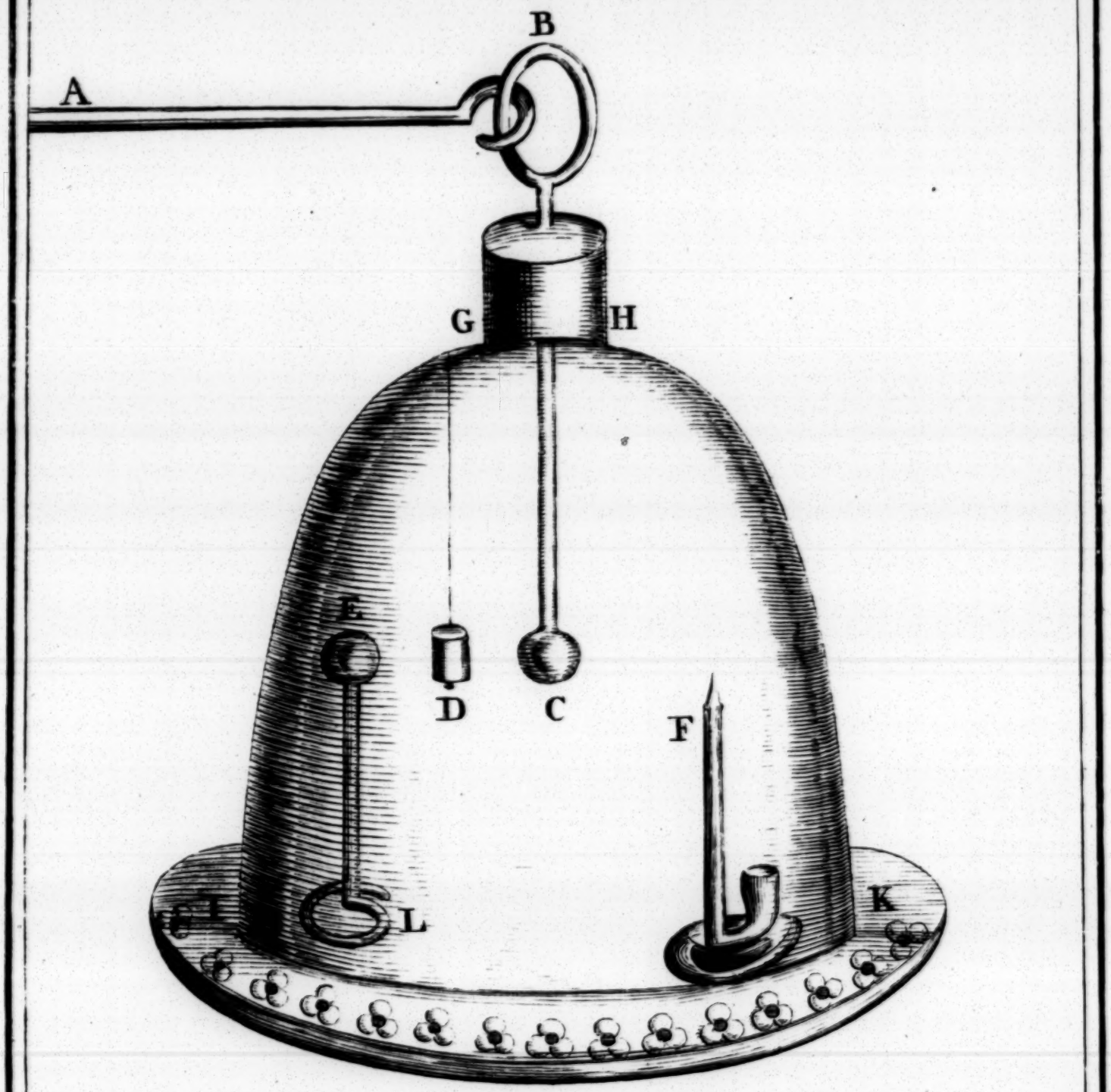
Read Feb. 14, 1760.

1. SOſpitem ex America Londinum te appuliſſe gaudeo, vir præclariſſime. Offero tibi de motibus electricis, qualem experimentis excudi, hypotheſin: partior hanc in duas partes, quemadmodum ipſa poſtulare videtur motuum hujuſmodi differentia: ago parte prima de acceſſionibus, de diſceſſionibus dico parte altera.

2. Et continuo univerſam de acceſſionibus pertractationem meam ita paucis comprehendo: " Quum " ignis electricus copioſior in corpore altero vi ex- " pandendi ſe ad æqualitatem trajicit in alterum, " partem interjecti aeris dimovet e loco pro majore " ſua copia majorem: fit inde, ut aer a tergo cor- " poribus adjacens deficiente fulcro aeris intermedii " ruat verſus ipſum medium locum; ruit vero non " circumeundo corpora, quæ via eſt longior, atque " impedita abs igne profluente, ſed ipſa trudendo a " tergo: hac aeris truſione accedunt corpora inæqua- " liter electrica; dum accedunt, accelerantur, quia " pro acceſſione majore copioſior ignis interfluit, co-

" pioſior

Phils. Trans. Vol. LI TAB. XII *p. 515.*



J. Mynde sc.

" piosior intermedius aer disjicitur, augeturque adeo " momentum aeris a tergo trudentis."

Experimentum I.

3. Aerem disjici ab interfluente electrico igne, scintillæ experimento demonstratur, quæ excitatur in medio tubulo, hinc clauso hermetice, inde impedito abs liquore aliquo, ipsa enim, disjiciens liquorem, disjectum aerum testatur.

4. Neque vero interfluens ignis aeris intermedii disjecti fulcrum potest supplere, quo aer corporibus a tergo adjacens sustineatur; is enim liberrime effluit, et effunditur per alterum corpus: quare cum non fulciatur ipse, nec fulcri vices potest agere.

Experimentum II.

5. Experimento res confirmatur plane luculentissimo: vittam ex charta inaurata longam pollices octo, latam lineas quatuor convolvo circa sese, ut cylindrum solidum D [*Vide Tab.* XII.] efformet: hunc suspendo serico filo DG sub fornice vitri pneumatici IHK, per cujus verticem H init virga metallica BC desinens in metallicam sphæram C: figo in lance IK virgam metallicam LE simili sphæra ornatam; nempe sphæræ C et E sunt in eodem plano cum pendulo D, atque ab ipso distant ambæ æqualiter; tum nexa catena AB, quæ pertinet ad machinam electricam virgæ BC, et consequenter immisso electrico igne in virgam BC, observo: 1. ante subductum aerem chartaceum cylindrum D agitari vehementissimis vibrationibus inter duas sphæras EC: 2. dum aer subducitur, minui eam agitationem plane pro copia subducti aeris: 3. omni, quantus potest, aere subducto, cylindrum D vix

nutare. His obſervatis aerem reſtituo particulatim; atque pro aere admiſſo video vibrationes iterum augeſcere, et fieri, quemadmodum antea, vehementiſſimas: quæ dum contemplor ſingula, atque omnia ſimul comprehendo animo, intelligo magnitudinem motuum electricorum aeri, vel toti, vel reſiduo proportione reſpondere.

Experimentum III.

6. Quod leviora corpuſcula, bracteolæ, et pulviſculi, motu adhuc ſatis ſenſili agitentur in rarefacto aere, id in cauſa fuit, cur cenſuerim a principio cum phyſicis aliis electricos motus actioni ignis electrici in aerem acceptos referri non debere; quod, re nunc accuratius collata, reſiduos videam in reſiduo aere motus non majorem habere ad primos motus, qui fiunt in toto aere atmoſpherico, proportionem, quam ea ſit, quæ inter reſiduum et primum aerem intercedit, id nunc facit, ut illum meum, et alienum errorem agnoſcam, atque in contrariam veniam, quam veriſſima teſtatur experientia, ſententiam.

7. Neque omittam partem obſervationis opportuniſſimam rei confirmandæ: ſoleo enim allatum ultimo loco experimentum perſæpe capere in cubiculo obtenebrato, atque tum obſervo ignem vibrantem pendulum ante aerem ſubductum micare hinc inde crepitantibus quidem, ac lucidiſſimis, ſed arctis, atque brevibus prope ſphæram utramque ſcintillis: deinceps ignem hunc pro ſubducto aere, adeoque pro minutis penduli vibrationibus effluere magis amplo radio, magis tranquillo, magis continuo, quanquam non ita micante.

8. Atque

8. Atque hæc ipſa res certe reactionem aeris demonſtrat ignem electricum cohibentis in denſitate quadam, quæ denſitati ſuæ proportioni reſpondeat.; hæc autem aeris reactio non poteſt actioni non reſpondere ignis electrici aerem nitentis disjicere, ac plane disjicientis, ubi neceſſaria is igni vi polleat.

9. Atque his plura non adferam tibi pro acceſſionibus explicandis corporum electricorum inæqualiter, attuli in datis ad Beccarium litteris, in quibus tamen non niſi dubitanter plura admodum differebam de mechanica acceſſionum cauſſa: accuratior conſideratio experimentorum facit nunc, ut de acceſſionum cauſſa dicam fidentius.

10. Corpora æqualiter electrica diſcedunt; qua cauſſa id efficiatur, nunc perſequar.

11. " Ignis electricus, qui quoquomodo inſidet in " corporibus, nititur, ut libretur cum igne electrico, " qui inſidet in aere. Si uterque" [finito me illum deinceps proprium dicere hunc aereum] " æqualem " habet proportionem ad nativam corporum atque " aeris capacitatem, corpora accedunt, aut in nativa " quieſcunt directione; alter ſi ſuperat proprius, vel " aereus, corpora explicat ſimiliter, compellitque ad " diſceſſionem." Hæc rei narratio.

Experimentum IV.

12. Quoties in cubiculo quodam meo amplo ipſo quidem at clauſo pergo diutius urgere alium, atque alium in catenam electricum ignem, experior partem ejus ignis effundi in aerem ambientem, ipſumque etiam, quantuſcumque in cubiculo eſt [dum a tempeſtate ſiccus ſit, vel abs igne] facere exceſſu electricum. Duo enim tenuia ex lino fila, quæ vel ſummis

digitis prehenſa, vel arundine ſuſpenſa defferò quolibet, in quodlibet cubiculi ſpatium, mutuo diſcedunt. Atque, ſive in eodem ipſa contineam loco, ſive in loco transferam alia atque alia, pergunt divergere ad ſemihoram, ad horam quandoque, et ultra.

Experimentum V.

13. Si ea fila ope vitri ſeparentur ab arundine, adeoque a ſolo, principio divergunt, tum ad nativam veniunt directionem: obtingunt autem hæc omnia poſt omnes elicitas, quantæ poſſunt, a catena ſcintillas, poſt omnem ab ipſa ſubductum redundantem ignem, poſt catenam nexam cum ſolo.

Experimentum VI.

14. Ex diuturna electricitate machinæ phænomena experior omnino eadem. At, ſi diuturna electricitas machinæ continuo conſequitur diuturnam electricitatem catenæ, aut contra, fila, quæ arundine ſuſpenſa conſtanter divergere videbam ex alterius electricitate, video principio accedere mutuo incipiente electricitate alterius, tum iterum divergere quemadmodum antea.

15. His inquam experimentis doceri nos arbitror.

" 1. Dum alium atque alium ignem congerimus in
" catenam, aliam atque aliam ipſius partem diſce-
" dere quam latiſſime in ambientem aerem, ipſique
" adhæreſcere, atque ita omnem contentum in cubi-
" culo aerem evadere exceſſu electricum: ſimiliter
" dum alium atque alium ignem ſubtrahimus a ma-
" china, aliam atque aliam ignis partem machinam
" haurire abs aere ambiente; atque ita omnem con-
" tentum in cubiculo aerem fieri defectu electricum.
" 2. Fila vel manu, vel arundine delata diſcedere

" in

" in aere, vel electrico exceſſu ex catena, vel electrico
" defectu ex machina, vel vi proprii nativi ignis ſu-
" perantis aereum, ſi merguntur in aere electrico ex
" machina, vel vi ignis aerei ſuperantis nativum pro-
" prium filorum ignem, ſi merguntur in aere electrico
" ex catena. 3. Fila hujuſmodi divergentia ex elec-
" tricitate catenæ, ex conſequente electricitate ma-
" chinæ (vel contra) primo accedere, quia machina
" primo hauriat abs aere exceſſum inditum a catena,
" vel catena ignem reſtituat aeri ſublatum a machina;
" atque ita ignis in aere ad nativam menſuram re-
" ductus librari debeat cum igne nativo permanente
" in filis. 4. At deinceps ex diuturniore electricitate
" machinæ conſequente electricitatem catenæ, vel
" contra, fila iterum divergere, quia vel machina per-
" gat haurire partem ignis nativi, vel catena pergat
" addere alium ultra nativum aeri ignem. 5. De-
" nique fila eadem cum vitro ſeparantur, principio
" divergere, quia ipſa, quæ ſolum ignem nativum
" habeant, mergantur in aerem vel exceſſu, vel de-
" fectu electricum. At dein, cum ſunt vitro ſepa-
" rata, venire ad nativam directionem, quia vel pars
" ignis aerei, ſi aer eſt exceſſu electricus a catena, in
" ipſa ſe denique effundat ad æqualitatem, quin ultro
" præterfluat ob cohibens vitrum; vel pars ignis na-
" tivi et proprii filorum ſe effundat in aerem, ſi aer
" eſt electricus defectu, quia ſuppleri aliunde poſſit
" ob interdicens vitrum."

16. Quare hæc videntur univerſe conſtare:
" 1. Aerem ſuum continere nativum ipſi ſibi ignem.
" 2. Nativo aeris igni ignem alium addi poſſe, na-
" tivi abs aere ignis partem poſſe ſubtrahi, ſed lente,
" et quaſi per vim. 3. Aerem dimittere redundan-

" tem,

" tem, haurire ignem deficientem, ſed quam lentiſ-
" ſime. 4. Si aer æque ac contenta corpora habeant
" nativum ignem ſuum, conſiſtere hæc in nativo
" ſtatu; ſi aer, manente nativo in corporibus igne,
" ignem habeat nativo aut copioſiorem, aut parcio-
" rem, corpora diſcedere." Verum ſingula hæc theo-remata experimentis aliis illuſtrari mirum in modum poſſunt, et demonſtrari latius.

Experimentum VII.

17. Duo fila nexa catenæ poſt paucas globi converſiones divergunt maxime; deinceps a diuturniore catenæ electricitate incipiunt minus divergere; atque hæc divergentia ad certum uſque gradum minuitur, ut quandoque angulus divergentiæ poſt continuatam diutius globi fricationem fiat ſubduplus anguli divergentiæ, qui extiterat ab electricitate eadem primo excitata. Quum hanc filorum divergentiam video minutam ſatis, tum globum non frico ulterius, ſed ſtilum acutiſſimum catenæ admoveo pedetentim, quo lente redundantem omnem abs ipſa ignem hauriam. Id dum fit, lente fila accedunt, tum continenter lente iterum divergunt ad angulum primo quidem minorem, ſed ſatis magnum, quæ quandoque vix metiantur gradus quadraginta quinque; tum vero pergunt divergere ad diuturnum tempus.

18. Videlicet maximus abs electricitate primo excitata angulus, quia tum ſatis ingens immiſſus in catenam, et in nexa fila, ignis; nullus, vel minimus, in aerem. Fit deinceps minor angulus, prout augeſcit ignis, qui conſequenter immittitur in aerem, conſtante eodem ignis exceſſu in catena. Dum admoto ſtilo lente minuitur ignis in catenam immiſſus, minuitur

nuitur adhuc angulus prout accedunt ad æqualitatem ignis in catena reſiduus, atque ignis in aerem immiſſus, ipſique affixus; ubi uterque ignis pertingit ad æqualitatem, fila attingunt ad directionem ſuam nativam; cum deinceps minuitur ulterius ignis redundans adhuc in catena, nec ſimiliter minuitur ignis redundans in aere [aeri enim, uti vidimus, adhæreſcit, atque ab ipſo lente dimittitur] fila iterum diſcedunt pro exceſſu ignis aerei ſupra ignem in catena; qui denique aereus ignis, quoniam lentiſſime dimittitur, diutiſſime pergunt divergere fila.

Experimentum VIII.

19. Si fila ſint nexa machinæ, eadem omnia in iis contingunt phænomena ex diuturna electricitate machinæ, quæ in iis contingebant catenæ nexis ex diutuna electricitate catenæ.

20. Quare univerſe, quemadmodum conſtitui a principio, " Si ignis proprius corporum, quantuſ-
" cumque is ſit, libratur cum igne ambientis aeris,
" corpora vel conſiſtunt in nativo ſtatu, vel ad ipſum
" redeunt; ſi alter ignis ſuperat alterum, corpora diſ-
" cedunt vi ignis ſuperantis."

21. Atque hinc rationem vides, ni fallor, vir præſtantiſſime, cur æque diſcedant tum quæ a catena, tum quæ a machina ſunt electrica corpora. " Ignis
" proprius in corporibus a catena electricis vincit
" aereum, aereus vincit æque proprium in electricis
" a machina."

Experimentum IX.

22. Quoties fila mutuo diſcedunt a ſe invicem, atque divergunt ex aucto in ipſis, vel minuto igne nativo

nativo ad meum accedunt admotum digitum: quoties diſcedunt, atque divergunt ſervantes ignem ſuum nativum ex aucto, vel minuto igne nativo aeris, a digito refugiunt meo.

23. Hac vero res primo quidem notiſſimis legibus acceſſionum atque diſceſſionum electricarum eſt conſentanea; quum enim nativus in filis ignis auctus eſt, vel minutus, ipſa ſunt inæqualiter ac digitus electrica, qui habet nativum ignem ſuum: quum vero nativus ignis perſtat in filis, et ipſa diſcedunt mutuo ob ignem auctum, vel minutum in aere; tum, quemadmodum diſcedunt ambo alterum abs altero, ita diſcedant oportet a digito meo ipſo etiam habente ignem nativum, vi aeris, in quo nativus ignis minutus eſt, aut auctus. Sed præterea hæc filorum a digito diſceſſio ipſa hanc probat etiam, quam affero, diſceſſionis cauſam: nam et digitus, et fila cum ſolo communicant, adeoque alium in ſe certe non habent ignem, niſi nativum, quo quoniam non diſcedunt, reſtat ipſa diſcedere ex nativo igne in aere aucto, vel minuto.

24. Cum in hanc primo incidi diſceſſionum electricarum theoriam, ea me ancipitem tenuit ſuſpicio, ne ignis aereus diſceſſiones facere non poſſet in machina iis æquales in pernicitate, et magnitudine, quas facit in catena ignis proprius: hæc inde ſubibat animum ſuſpicio, quod ignis in machina, et catena (univerſe in corporibus omnibus deferentibus) menſura mutetur quam perniciſſime, lentiſſime autem augeatur, aut minuatur menſura ignis in aere ambiente. Alia præterea ſuſpicionem exagerabat conſideratio; tantum me nunquam abs aere, quantum a machina, ſubtrahere ignem potuiſſe, tantum potuiſſe nunquam aeri addere, quantum catenæ; nunquam enim eo veni,

ut

ut omnem aerea ſola electricitate ſuſtulerim filorum divergentiam, quanta exiſtebat ex integra machinæ aut catenæ electricitate.

Factum inde eſt, ut experimenta hæc eadem, imo et alia, in literis attulerim ad præclariſſimum inſtituti Bononienſis præſidem datis, quin hanc in iis ſatis late theoriam diſceſſionum conſtituerem.

Verum re maturius perpenſa intellexi pernicitatem diſceſſionum, quæ fiant abs igne aereo, non ipſi pernicitati reſpondere debere, qua vel augeatur ipſius copia, vel minuatur; ſed pernicitati, qua agat copia inhærens; et magnitudinem diſceſſionum, quæ fiant ab igne eodem, non abſolutæ, ſed comparativæ ipſius magnitudini reſpondere debere; utrumque autem facile vidi pro re obvenire.

Experimentum X.

25. Si in experimento VI. poſt minutam ex diuturniore electricitate catenæ filorum divergentiam, non lente, ſed protinus (manu nempe in catenam immiſſa) omnem ſubtraho a catena redundantem ignem, continuo incipiunt quidem accedere fila quam velociſſime; verum velocitate æquali reſiliunt repente, antequam pertingant ad nativam directionem. Hæc autem altera diſceſſio quoniam fit ex aereo igne ſuperante proprium in filis reſiduum, video inde ignem aereum (quamquam mutetur lentiſſime ipſius copia) ipſum tamen diſceſſiones facere iis æque veloces, quæ abs igne proprio efficiuntur.

Experimentum XI.

26. Sed et ipſas ex igne aereo obvenire æque magnas ita intelligo. Poſt ignem inditum in aerem, qui

catenam ambit quantus poteſt, dempto redundante omni ab catena igne, atque adeo diſcedentibus jam filis ex igne aereo, vitreo globo ſubſtituo ſulphureum, atque ex hujus functione, video, principio augeri angulum divergentiæ, quem effecerat ignis aereus, tum ipſum continenter minui.

27. Id autem certum: primas globi ſulphurei frictiones demere partem nativi ignis a catena, neque pariter demere exceſſivum ignem ambientis aeris (aer enim lente, ut vidimus, accipit ignem alienum, lente acceptum exuit); quare augeſcit magnitudo divergentiæ, quæ fit abs igne aereo, non mutata magnitudine abſoluta ignis aerei, ſed mutata ſolum magnitudine ipſius comparativa, minuto nempe igne proprio; adeoque aucta proportione aerei ad proprium. Igitur quoniam quantum ignis ingeritur in catenam, tantundem ſubtrahitur a machina; proportio ignis aerei circa machinam ad reſiduum in machina eadem erit ac proportio ignis in catena redundantis ad aereum circa catenam: quamobrem aereus circa machinam diſceſſiones faciet non æque veloces ſolum, ſed et æque magnas, ac eæ ſunt, quas facit ignis proprius in catena.

28. Itaque, ut omnia demum paucis complectar, quæ pertinere videntur ad motus electricos univerſe omnes explicandos (ſuſpenſiones enim, adhæſiones, vibrationes, infinitoſque alios compoſitiores quaſi ludos electricos, tu ipſe probe videre viſus es diſceſſionibus omnes, atque acceſſionibus contineri) hæc denique eſt ſumma hypotheſis meæ. " Acceſſiones cor-
" porum inæqualiter electricorum efficiuntur ab igne
" electrico a corpore altero, in quo copioſior eſt, ef-
" fluente in alterum per aerem interjectum, ipſum-
" que

" que disjiciente. Discessiones autem vel igne pro-
" prio corporum efficiuntur se expandente contra
" aereum, vel aereo se expandente valentius contra
" proprium;" quæ tamen expansio ignis alterius alterum superantis sine mixtione mutua alterius cum altero videtur contingere. Aer enim ignem continet suum quantuncumque, ne discedat; arcet proprium deferentium corporum ignem, ne adhæresçat sibi.

29. Hanc, inquam, hypothesim offero tibi, vir præclarissime, quo tu eam facias meliorem. Si tanti interea ipsam ducas, ut Regiæ isti scientiarum Societati exhibenda videatur, res ex meo obtinget desiderio, qui quando ornamentum ipsi adferre nullum possum, diligentiam saltem nolim desiderari meam. Tu vero cura, ut valeas. Servari enim te decet quam diutissime utilissime, scientiæ perficiendæ amplificandæque, quam certissimam instituisti. Dat. Taurini, 24 Decembris, 1757.

Remarks on the preceding Paper, by Benjamin Franklin, *L. L. D. F. R. S.*

Read Feb. 14, 1760. FOR the better understanding this paper, it is necessary to know, that Father Beccaria uses a large chain, suspended by silk lines, for the purpose of a prime conductor; and that his machine for turning the glass globe is so contrived, as that he can, on occasion, readily isolate it; (*i. e.* place it on glass or wax) together with the person that works it. When the communication is thus cut off between the earth and the chain, and also between the earth and the machine, he observes, that the globe being turned, both the chain and the

machine ſhow ſigns of electricity; and as theſe ſigns, when examined, appear to be different in the chain, and in the machine, and the globe having, as he ſuppoſes, drawn from the machine part of its natural or common quantity of electricity, and given it to the chain, he calls the electricity appearing in the chain, electricity *by exceſs*; and the electricity appearing in the machine, electricity *by defect*; which anſwer to our terms of *poſitive* and *negative* electricity, or electricity *plus* and *minus*. And thus his expreſſions, *electrifying by the chain*, and *electrifying by the machine*, are to be underſtood, electrifying *poſitively*, and electrifying *negatively*.

LI. *An uncommon Caſe of an Hæmoptyſis*; *by* Eraſmus Darwin, *M. D.*

To the very Honourable and Learned the Preſident, and Members of the Royal Society.

Gentlemen,

Read Feb. 14, 1760. THE following caſe of a diſcharge of blood from the pulmonary artery, appears to have been owing to a cauſe different from any mentioned amongſt the writers of medicine: and as, from the knowlege of that cauſe, the cure was ſo eaſily deduced, I flatter myſelf, you will not eſteem it unworthy your attention.

A gentleman, reſiding near this place, between forty and fifty years of age, of a pale and meagre habit, has been daily afflicted with violent head-achs for

for ſeveral years paſt; and, about four years ago, after having taken a conſiderable quantity of Peruvian bark, became ſuddenly paralytic. The uſe, however, of his right limbs was ſo much reſtored, as only to remain weaker than the other; when, upon ſuddenly awaking from his ſleep about two o'clock in the morning, (May 7, 1759), he ſpit up four or five ounces of florid blood.

He immediately loſt twelve or fourteen ounces from the arm, had elixir of vitriol given him, and in the evening had a glyſter, and loſt blood again to about ten ounces.

On the 8th, about the ſame hour, he again ſuddenly awaked, and ſpit about the ſame quantity of blood as before. He was now adviſed to increaſe the quantity of elixir of vitriol, had a bolus of *extractum Campechenſe* every ſix hours, and had a leech applied to a blind pile, that had long appeared after going to ſtool.

On the 9th, at the ſame hour, he had again the ſame diſcharge as before. That theſe hæmorrhages were from the pulmonary artery, rather than the bronchial, appears from the ſudden exſpuition, the quantity, the floridity, and from the diſcharge being without pain, and unmixed with phlegm.

As he had no feveriſh ſymptoms, either when he firſt awaked, or during the day, no more blood was taken from him; and as he conſtantly ſlept profoundly from ten o'clock till two, when the complaint ſeized him, he was now adviſed to be awakened, and riſe out of his bed, at one in the morning, and remain awake till three, omitting all medicines.

He

He continued to riſe from bed for a week, and has ever ſince uſed himſelf to awake at the ſame time; and has not only been entirely free from this complaint, and that without any further diſcharge from the hæmorrhoidal veſſels; but has got more fleſh, and his head-achs are become even inconſiderable.

I ought not here to omit, that he had a vomit given him on the 12th, and twice repeated, at the intervals of three or four days.

As the patient, from a former hæmiplagia, had, in all probability, many parts of his body rendered leſs irritable than is natural; and as he conſtantly ſlept profoundly, and the hæmoptoe always awaked him after four hours ſleep; I was led to conclude, that, during this ſleep, the lungs were not ſufficiently ſenſible to puſh forwards the whole circulation; and and that hence the blood, gradually accumulated, ruptured ſome minute branches of the pulmonary artery, before the uneaſineſs became great enough to awake the patient. And, as much as the evidence of a ſingle caſe in medicine may be eſtimated, the ſucceſsful cure would ſeem to evince the truth of this doctrine.

I have only to add, that the anxiety, with which patients reduced to great weakneſs awake from their ſleep, and the hurried pulſe, have, by others, been obſerved to be owing to an accumulation of blood in the lungs, during their ſtate of decreaſed ſenſibility: And how detrimental, in theſe caſes, might be the adminiſtration of opium, or nitre; whilſt the want of ſleep, or the recurring hæmorrhage, might ſeem, to the unwary practitioner, to need their aſſiſtance.

After a few days, obſerving ſome cough remain, it ſeemed adviſeable to give two or three vomits; as, from late experience, they do not endanger a renewal of the diſcharge, and muſt promote the expectoration of the eſchar, or any extravaſated blood; which otherwiſe, by its delay acquiring a putrid acrimony, perhaps moſt frequently erodes the contiguous veſſels, and, forming new ulcerations, becomes the general cauſe of conſumptions, ſubſequent to accidental ſpittings of blood.

I am, Gentlemen,

Your very humble ſervant,

Lichfield, Dec. 17. 1759. Eraſmus Darwin.

LII. *An Account of the late Earthquakes in* Syria: *In a Letter from Dr.* Patrick Ruſſell, *to his Brother,* Alexander Ruſſell, *M. D. F. R. S.*

Aleppo, Dec. 2, 1759.

Read Feb. 21, 1760.

AS I recollect nothing in the way of buſineſs, which I have not already wrote you about; and as I know not when this letter may get away, I ſhall, in the mean time, give you ſome account of the earthquakes here, which have thrown the people into a terrible conſternation.

The ſpring of this year was unuſually dry, the ſummer temperate, and the autumn, though the rains came on towards the end of September, might be

be eſteemed much drier than in other years. The Aleppo river has been very low all the ſummer; and its bed, from the firſt to the ſecond mill, is, I believe, even now ſtill without water. This phænomenon I, at firſt, thought remarkable; but have been informed, that the ſcarcity of water complained of, during all the ſummer, was occaſioned by driving the river into ſome rice-grounds lately formed towards Antab.

On the morning of the 10th of June, a ſlight ſhock of an earthquake was felt here, and, as uſual, ſoon forgot; having, ſo far as we know, been felt in no other place, in any degree of ſeverity.

October the 30th, about four in the morning, we had a pretty ſevere ſhock (indeed the moſt violent I had ever felt), which laſted ſomewhat more than a minute, but did no damage in Aleppo. In about ten minutes after this firſt, there was a ſecond ſhock; but the tremulous motion was leſs violent, and did not laſt above fifteen ſeconds. It had rained a little in the preceding evening; and when the earthquake happened, the weſt wind blew freſh, the ſky was cloudy, and it lightened.

This earthquake occaſioned little alarm amongſt the natives, and even with the Europeans was the topic only for a day. But the ſubject was ſoon revived, by letters from Damaſcus, where the ſame ſhock felt by us at Aleppo, and ſeveral other ſucceſſive ones, had done conſiderable damage. From this time, we had daily accounts of earthquakes from Damaſcus, Tripoly, Seidon, Acri, and all along the coaſt of Syria; but ſo exaggerated in ſome circumſtances, and ſo inaccurate in all, that we only knew

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in general, that Damaſcus, Acri, and Seidon, have ſuffered injury from the earthquake, though leſs than was at firſt given out.

Such ſtories circulating among the people, rendered them more ſuſceptible of alarm; and an occaſion ſoon offered, where they feared, that the worſt of their apprehenſions were to be realized.

This happened the 25th of November. The morning had been ſerene, ſome clouds aroſe after noon, and the evening was remarkably hazy, with little or no wind. About half an hour after ſeven at night, the earthquake came on: the motion, at firſt, was gently tremulous, increaſing by degrees, till the vibrations became more diſtinct, and, at the ſame time, ſo ſtrong, as to ſhake the walls of the houſes with conſiderable violence; they again became more gentle, and thus changed alternately ſeveral times during the ſhock, which laſted in all about two minutes. In about eight minutes after this was over, a ſlight ſhock, of a few ſeconds duration, ſucceeded. The thermometer was at 50, and the barometer ſtood at 28-9, the mercury ſuffering no alteration. There was little or no wind in the night, the ſky clear, excepting ſome heavy clouds, that hung about the moon. At a quarter after four next morning, we had another ſhock, which laſted ſomewhat leſs than a minute, and was hardly ſo ſtrong as that of the preceding night. The night of the 26th was rainy and cloudy. At nine o'clock we had a ſlight ſhock, of a few ſeconds. The motion here appeared to be very deep, and was rather undulatory than tremulous. The 27th, cloudy and rainy. From midnight of the 25th, beſides theſe now mentioned, four or five ſlighter

ſhocks were felt; but I myſelf was ſenſible of none; till the morning of the 28th, when we had a ſhort pulſatory ſhock. The ſame day, at two o'clock, we had a pretty ſmart ſhock, laſting about forty ſeconds. From this time, I was ſenſible of no more, though others either felt or imagined ſeveral ſlight vibrations every day.

However violent this earthquake was, or rather, by the frightened people of Aleppo, imagined to be, it is certain, that, excepting in a very few old walls, the city bears no freſh marks of ruin; none of the oldeſt minorets have ſuffered. Its effects at Antioch were more formidable; many houſes have been thrown down, and ſome few people killed.

December 7 h.

THE earthquake of the evening of the 25th has proved fatal to Damaſcus; one-third of the city was thrown down, and of the people, numbers yet unknown periſhed in the ruins. The greater part of the ſurviving inhabitants fled to the fields, where they ſtill continued, being hourly alarmed by ſlighter ſhocks, which deterred them from re-entering the city, or attempting the relief of ſuch as might yet be ſaved, by clearing away the rubbiſh. Such was the purport of a letter I read this day, which was wrote from Damaſcus three days after the earthquake. Other accounts we have at this place, make the loſs of the inhabitants amounts to 20,000; but, in circumſtances of ſuch general horror and confuſion, little accuracy can be expected, and the eaſtern diſpoſition to exaggeration reigns, at preſent, univerſally.

Tripoly has ſuffered rather more than Aleppo; three minorets, and two or three houſes, were thrown down,

down, while the walls of numbers of the houſes were rent. The Franks, and many of the natives, have deſerted the city, and remain in the fields.

At Seidon, great part of the Frank kane was overthrown, and ſome of the Europeans narrowly eſcaped with their lives. Acri and Latakea have ſuffered little, beſides rents in ſome of the walls: but Saphet (eight hours from Acri) was totally deſtroyed, together with the greater part of the inhabitants.

Such are the moſt authentic accounts we have from abroad; how much they contribute to calm the minds of the people at Aleppo, you may eaſily imagine. Since the afternoon of the 28th, ſeveral ſlight ſhocks have been every day felt, and many more formed by the power of imagination: for my own part, I have ſince that time perceived none, excepting one the evening of the 5th, which was pretty ſtrong, but laſted not above twenty ſeconds. The weather, for theſe two days, has been gloomy and rainy; a change which people are willing to flatter themſelves is favourable. It often lightens in the night, and thunder is heard at a great diſtance.

Extract from another Letter of Dr. Patrick Ruſſell, *to Dr.* Alexander Ruſſell, *dated at* Aleppo, 29 March 1760.

IN my laſt, of the 7th of December, I gave you a full account of the earthquakes, which have occaſioned an univerſal panic all over Syria. There were ſeveral other ſhocks in December, and a few very ſlight ones in January: ſince which time, all has been quiet.

Excepting a few old houſes, none of the buildings in Aleppo were actually thrown down; but the walls of a conſiderable number of houſes have been rent.

We have had the wetteſt winter I ever ſaw in this country. The Coic has every-where overflowed its banks, and is juſt now much higher than it has been known to riſe for many years.

LIII. *Remarks on the* Bovey *Coal: In a Letter to the Right Honourable* George *Earl of* Macclesfield, *Preſident of the Royal Society.* *By* Jeremiah Milles, *D. D. F. R. S.*

My Lord,

Read Feb. 28, 1760. THE deſcription, which the learned profeſſor Hollman has given the Society *, of two remarkable ſtrata of foſſil wood in Germany, one in the neighbourhood of Munden, in the duchy of Grubenhagen, and the other near Allendorf in Heſſe, correſponds, in ſo many particulars, with ſome ſtrata, diſcovered about fifteen years ago, in Devonſhire, that it ſuggeſted to me a doubt, whether thoſe German ſtrata were really (what the learned profeſſor ſuppoſes them) foſſil wood, and formerly a vegetable ſubſtance, or (what he ſays the miners call them) foſſil coal. The reaſons, on which this doubt is founded, are ſubmitted to the judgment of your Lordſhip, and the Society, in the following

* See the above, N° xlix. p. 506.

account of the Devonſhire foſſil, which is commonly known by the name of the Bovey coal. It is found on a common ſurrounded with hills, called Bovey Heathfield, in the pariſh of South-Bovey, 13 miles ſouth-weſt of Exeter, and 3 miles weſt of Chudleigh. The uppermoſt of theſe ſtrata riſes within a foot of the ſurface, under a ſharp white ſand, intermixed with an aſh coloured clay, and underlies to the ſouth about 20 inches in a fathom.

The perpendicular thickneſs of theſe ſtrata, including the beds of clay, with which they are intermixed, is about 70 feet. There are about ſix of each, and they are found to continue eaſtward, in an uninterrupted courſe, to the village of Little-Bovey, a mile diſtant, and probably extend much further. The ſtrata of coal near the ſurface are from 18 inches to 4 feet thick, and are ſeparated by beds of a browniſh clay, nearly of the ſame dimenſions, but diminiſhing in thickneſs downwards, in proportion as the ſtrata of coal grew larger; and both are obſerved to be of a more compact and ſolid ſubſtance in the lower beds. The lowermoſt ſtratum of coal is 16 feet thick; it lies on a bed of clay, under which is a ſharp green ſand, not unlike ſea ſand, 17 feet thick, and under that, a bed of hard cloſe clay, into which they bored, but found no coal. From the ſand ariſes a ſpring of clear blue water, which the miners call mundic water, and a moiſture of the ſame kind trickling through the crevices of the coal tinges the outſide of it with a blue caſt.

Some ſmall and narrow veins of coal are found intermixed with, and ſhooting through, the beds of clay, forming impreſſions like reeds and graſs, and very

very ſimilar to thoſe, which are generally found on the top of coal mines. The clay alſo (at leaſt that part of it, which lies neareſt to the coal) ſeems to partake of its nature, having ſomewhat of a laminous texture, and being in a ſmall degree inflammable; and amongſt this clay, but adhering to the veins of coal, are found lumps of a bright yellow loam, extremely light, and ſo ſaturated with petroleum, that they burn like ſealing wax, emitting a very agreeable and aromatic ſcent.

Though the ſubſtance and quality of this coal, in its ſeveral ſtrata, are much alike, and it is all indiſcriminately uſed for the ſame purpoſes; yet there is ſome difference in the colour, form, and texture, of the ſeveral veins. The exterior parts, which lie neareſt to the clay, have a greater mixture of earth, and are generally of a dark brown, or chocolate colour; ſome of them appear like a maſs of coal and earth mixed; others have a laminous texture, but the laminæ run in ſuch oblique, waving, and undulating forms, that they bear a ſtrong reſemblance to the roots of trees, of which kind I have ſeen ſome ſpecimens from Lough Neagh in Ireland, which ſeem to be the ſame ſort of foſſil.

There are other veins of this coal, which lie more in the center of the ſtrata, and abound moſt in the loweſt and thickeſt bed, the ſubſtance of which is more compact and ſolid: theſe are as black, and almoſt as heavy, as pit coal; they do not ſo eaſily divide into laminæ, and ſeem to be more ſtrongly impregnated with bitumen: theſe are diſtinguiſhed by the name of ſtone coals, and the fire of them is more ſtrong and laſting than that of other veins.

But

But the moſt remarkable and curious vein in theſe ſtrata is that, which they call the wood coal, or board coal, from the reſemblance which the pieces have to the grain of deal boards. It is ſometimes of a chocolate colour, and ſometimes of a ſhining black. The former ſort ſeems to be leſs impregnated with bitumen, is not ſo ſolid and heavy as the latter, and has more the appearance of wood. It lies in ſtrait and even veins, and is frequently dug in pieces of three or four feet long, and, with proper care, might be taken out of a much greater length. Other pieces of the ſame kind are found lying upon them, in all directions, but without the leaſt intermixture of earth, or any other interſtices, except ſome ſmall crevices, by which the pieces are divided from each other, in all directions. When it is firſt dug, and moiſt, the thin pieces of it will bend like horn, but when dry, it loſes its elaſticity, and becomes ſhort and criſp. At all times, it is eaſily to be ſeparated into very thin laminæ, or ſplinters, eſpecially if it lie any time expoſed to the heat of the ſun, which, like the fire, makes it crackle, ſeparate, and fall to pieces. The texture of this foſſil conſiſts of a number of laminæ, or very thin plates, lying upon each other horizontally, in which ſmall protuberancies ſometimes appear, like the knots of trees; but, upon examination, they are only mineral nuclei, which occaſion this interruption in the courſe of the laminæ; and pieces of ſpar have been ſometimes found in the middle of this wood coal.

Though the texture of this coal is laminated, yet it does not appear to have any of thoſe fibrous interſections, which are obſerved in the grain of all wood.

This

This coal eaſily breaks tranſverſely, and the ſeparated parts, inſtead of being rugged and uneven, are generally ſmooth and ſhining, in which even the courſe of the laminæ is hardly diſcernable.

They dig this coal in an open pit, together with the clay that is mixed with it; and though it lies very cloſe and compact in its original bed, yet it is ſo eaſily ſeparated, that they can afford to ſell it for half a crown a ton at the pit. The ſmaller coal is ſeparated from the clay by a ſkreen, or grated ſhovel; the larger, which riſes ſometimes in pieces of above an hundred weight, is piled up by hand. There is hardly any other uſe made of it at preſent, but to bake the earthen ware of a manufacture erected at South-Bovey, and for burning of limeſtone, which riſing in great quantities at the neighbouring town of Chudleigh, the coal is carried thither, and they return with limeſtone to the pit; which they burn there, for the uſe of the northern pariſhes, to whom it lies more convenient than the kilns of Chudleigh.

The fire made by this coal is more or leſs ſtrong and laſting, according to its different veins: thoſe which lie neareſt to the clay, having a greater mixture of earth, burn heavily, leaving a large quantity of browniſh aſhes; that, which they call the wood coal, is ſaid to make as ſtrong a fire as oaken billets, eſpecially if it be ſet on edge, ſo that the fire, as it aſcends, may inſinuate itſelf between, and ſeparate the laminæ. But that of the ſtone coal is accounted moſt ſtrong and durable, being apparently more ſolid and heavy, and probably alſo more ſtrongly impregnated with bitumen. One of the proprietors of this coal made an experiment of burning it in the fire-engines of Cornwall,

wall, to which it might have been tranſported, without much trouble or expence, being only four miles diſtant from the navigable river Teign; but the heat of the coal was not ſufficiently intenſe, and the conſumption of it too great, to anſwer the purpoſe. When this coal is put into the fire, it crackles, and ſeparates into laminæ, as the cannel coal does into irregular pieces, burns for ſome time with a heavy flame, becomes red-hot, and gradually conſumes to light white aſhes. Though the tranſverſe crevices made in it by the fire give it the external appearance of a wooden brand, yet, if quenched when red-hot, the unconſumed part does not look like charcoal, but ſeems to be almoſt as ſmooth and ſolid, as when firſt put into the fire.

The thick heavy ſmoak, which ariſes from this coal when burnt, is very fœtid and diſagreeable; entirely different from that aromatic ſcent of the bituminous loam, which is found adhering to it, but much reſembling that of the aſphaltum, or bitumen of the Dead Sea. The whole neighbourhood is infected with the ſtench, which is wafted by the wind to the diſtance of three or four miles. When burnt in a chimney (as it is ſometimes in the neighbourhood), the offenſiveneſs is leſſened by the draught: however, it is found, by thoſe, who live continually in the ſmoak of it, not to be unwholſome; nor is it in the leaſt prejudicial to the eyes, like the ſmoak of wood. The moſt ſhining and ſolid pieces of this coal have not the leaſt degree of electrical attraction.

Notwithſtanding the reſemblance, which this foſſil bears to wood, eſpecially when viewed in detached pieces, yet the following obſervations on its ſituation,

its form and properties, will prove it to be not of a vegetable, but of a mineral origin.

In the firſt place, there does not ſeem to be any imaginable cauſe in nature, which could bring together ſuch a maſs of foſſil wood, as is found in this, and other ſtrata of the like kind in different parts of Europe. It extends here to the depth of 70 feet: in that near Munden they have ſunk 50 feet, without coming to the bottom. Foſſil trees, though frequently found ſingle, or in ſmall numbers, are generally diſcovered in moraſſes and ſoft ground, where they have either buried themſelves by their own weight, or been overwhelmed by ſome accidental cauſe: but the Bovey ſtrata are found in a dry ſoil, intermixed with clay and ſand, and, by their regular courſe and continuance, carry the moſt undoubted marks of never having been diſturbed ſince their original formation. Foſſil trees likewiſe preſerve their form and ſize, their length and roundneſs, their branches and roots, their fibrous texture and ſtrength, and are either found entire, or in ſuch large pieces, that there is no room to doubt of their nature, ſince the very ſpecies of wood is frequently diſtinguiſhable in them; whereas the Bovey coal comes out only in flat pieces, of a few feet long, like the ſplinters of large maſts; and on them we diſcover no ſigns of roots, branches, or bark, no round pieces, or concentric circles, which diſtinguiſh the annual growth of trees; the laminæ, which have the appearance of wood, being always horizontal, according to the ſituation of the pieces in the ſtrata: or could we ſuppoſe a number of foſſil trees to be brought together, and ranged in this regular manner in the ſeveral

ſtrata,

ſtrata, yet, by the form and roundneſs of their trunks, they muſt be, in a great meaſure, encompaſſed by the ſoil, in which they are buried; whereas there is not the leaſt mixture of earth, or any other aperture, in the Bovey ſtrata of coal, except a few crevices, common to this ſort of foſſil, which divide the pieces from each other in all directions, and ſeem to be inconſiſtent with the nature and fibrous texture of wood.

If the baſis or matrix of this foſſil were wood, it would acquire, by being impregnated with bitumen, a greater degree of inflammability; whereas it neither kindles nor conſumes ſo faſt as wood.

The inflammability and laminated texture of this foſſil, which have been the only reaſons for ſuppoſing it of vegetable origin, may be accounted for from the nature of its principles, and their diſpoſition, when united, to aſſume certain forms. The bituminous matter uniting with various kinds of earth, in certain proportions, will form an inflammable ſubſtance; and a ſimilar mixture of theſe will generally aſſume a ſimilar texture and form, as will appear from the accounts given us of this foſſil both by the antients and moderns, moſt of whom ſpeak of it as a mineral ſubſtance.

Theophraſtus * mentions " a ſtone on the pro-
" montory of Erineas in Peloponeſus, and another
" in the mines of Binæ in Thrace, which being
" burnt, emitted a bituminous odour; and another
" ſtone in the mines of Scaptiſulæ, not unlike to rotten

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* De Lapidibus, p. 4.

" wood, which would burn, if oil was poured on it, " but would ceaſe burning as ſoon as the oil was " conſumed."

Dioſcorides * obſerves of the lapis gagates, which is certainly a ſpecies of this foſſil, " that the ſort, " which kindles quickly, and gives a bituminous " odour, is the beſt." He deſcribes it as generally of a black colour, and uneven ſubſtance, broad and flat, and very light. He mentions † alſo a black earth, found at Seleucia in Syria, called terra Ampelitis, which reſembled long pieces of foſſil coal, ſplitting eaſily, of an uniform gloſſy ſuperficies, which being pounded, and immerſed in oil, immediately liquified.

Pliny ‡ deſcribes the gagates as black, porous, and flat, not much unlike wood, brittle, of a diſagreeable ſmell when rubbed, and emitting a ſulphureous odour when burnt.

Galen ‖ calls it " a black ſtone, which, when put " in the fire, ſends forth a bituminous odour." And he ſpeaks of ſome other black ſtones, brought by him out of Cœloſyria, " which were broad like a " board, and, being put into the fire, burnt with a " ſlender flame: they were generated in the hills on " the eaſt ſide of the Dead Sea, where the bitumen " is produced, and the ſmell of the ſtone was like " bitumen." I cannot but obſerve on this paſſage, that the ſmell of the Bovey coal, when burnt, is the

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* Lib. v. cap. 103.
† Lib. v. cap. 138.
‡ Hiſt. Nat. lib. xxxvi. cap. 19.
‖ De Simplicium Medicamentorum Facultat. lib. ix. de Gagate.

ſame

ſame with that of the aſphaltum, and of a ſtone impregnated with it, which is found at a ſmall diſtance from the Dead Sea.

Agricola * mentions theſe, and ſeveral other kinds of bituminous foſſils, taken notice of by the antients, and points out ſeveral places in Europe where they are now found. Later writers deſcribe it under the name of metallophyton, or lignum foſſile; and moſt of them ſuppoſe it to be a mineral ſubſtance. Franciſco Stelluti, in a treatiſe printed at Rome 1637, has accurately deſcribed ſome very curious ſtrata of this kind, near Todi and Aqua Sparta in Umbria: he ſays, "they conſiſt of large oval and compreſſed pieces, "lying horizontally, reſembling the trunks of trees, "but larger: he mentions one above 3 feet broad: "they have neither roots, branches, nor fibres, but "ſomething like bark; and the heart of the tree, "which in ſome is as hard as a bone, in others as "black as a coal. The extremity of theſe pieces is "as ſmooth, as if they had been divided by a ſaw. It "is full of veins of a dark brown colour, which, inſtead of running ſtrait like the fibres of wood, are "undulated and irregular, taking a variety of forms, "and are only ſuperficial; for, upon taking off one "of the thin laminæ, of which this foſſil is compoſed, the veins underneath appear in a different "direction. This foſſil, when firſt taken out of the "earth, bends like a bow; and when put into the "fire conſumes ſlowly, with a great ſmoak, and diſagreeable ſmell; but, if burnt when dry, the ſmell "is more pleaſing. The heat is more intenſe than

* De Natura Foſſilium, lib. iv. p. 596.

"wood,

" wood, and it does not conſume ſo faſt. Many of " theſe pieces are ſprinkled with marcaſite, and there " exſudes from it a white bituminous ſubſtance, like " roſin. It is full of crevices, in all directions, and the " pieces appear in a variety of forms," which the author has exhibited, together with a repreſentation of the veins of the foſſil, in ſeveral copper-plates accompanying his work. He is clearly of opinion, " that this " ſubſtance is not generated from the ſeed or roots of " any plant, but from a cretaceous earth transform- " ing itſelf into wood, by the aſſiſtance of ſulphu- " reous water minerals, and ſubterraneous heat, " which appears there in a thick ſmoak, and ſome- " times in flames, particularly in rainy weather." He forms his ſuppoſition on the different appearances of this foſſil, " ſome pieces of it being like " chalk, others like wood, and again others like " coal; ſome are either totally or partially petrified, " being ſtone without and wood within, or *vice* " *verſa*." But theſe various appearances may be eaſily accounted for, from the different kinds and proportions of earthy matter, with which the bitumen is mixed.

Of the ſame kind are the foſſil ſtrata in Iceland, mentioned in the Muſæum Wormianum, and by Horrebow, in his natural hiſtory of that iſland. The former of theſe authors * deſcribes it as " a laminated " ſubſtance, generally of a black, but ſometimes only " of a dark colour, heavy, and, when dry, brittle; " though the merchant, who gave the ſpecimen, " aſſured him, that when it was firſt dug out of the

* Lib. ii. cap. 16. p. 169.

" earth

" earth it would bend like a twig. It conſiſts of " oblique fibres, and here and there ſome knots " like the roots of a great tree. A carpenter, who " was a judge of wood, thought it to be the root " of a walnut-tree grown black with age. It was " not eaſily to be poliſhed, too brittle to be uſed " in cabinet work, and was full of crevices. This " ſtratum is found ſome yards under the earth, in a " mountain ſo high and perpendicular, that thoſe " only who have been accuſtomed to climb ſuch " precipices can venture to dig for it. There is not " the leaſt appearance, that trees ever grew where this " foſſil is found, notwithſtanding this author ſup- " poſes them to be roots of trees turned black, by a " ſubterraneous vitriolic juice. The duſt of this ſtra- " tum is accounted, by the Icelanders, an excellent " preſervative of cloaths from moths and worms."

Horrebow * ſays, " it is an extraordinary ſort of " wood, which they call ſortebrand, or black brand, " very hard, heavy, and black like ebony. It is " found deep in the ground, in broad, thin, and " pretty large pannels, or leaves, fit for a moderate " ſized table. It is generally wavy, undulated; and " is always found between the rocks, or great ſtones, " wedged, as it were, cloſe in." He was doubtful, whether it were wood, or a petrification; but inclined to the former opinion, becauſe it could be planed and managed like wood.

The ſame ſort of foſſil, according to Wormius †, " is found in the iſlands of Faro. It does not eaſily

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* Page 33.

† Muſæum Wormianum, p. 31. cap. 13. de Bitumine.

" take

" take fire, but has a ſplendor like gagates. It is " found in the joints of the rock, and is taken out " in laminæ, or ſplinters, of 3 or 4 inches thick."

Of the like kind is the ſtratum of foſſil wood near Thun in Swiſſerland, mentioned by Scheuchzer, in his Itinera Alpina *; which he deſcribes as lying under ſeveral ſtrata of flints, clay, and aſh coloured marle. Being expoſed to the air, parts of it grew hard, and others broke to pieces. " It is (he ſays) " obſervable of this foſſil wood, that the trunks and " branches are not round, but compreſſed; yet, in " ſome places, cloathed with their bark, and here " and there adorned with their leaves. The wood " is inflammable, making a ſtrong fire, and ſerves " inſtead of foſſil coals." The author ſuppoſes the compreſſion of this ſtratum to be owing to the great ſuperincumbent weight; but others, he ſays, imagine it to be ſo formed by nature, from clay, in the bowels of the earth. I would obſerve here, that the cruſtaceous appearance, ſo common to theſe bituminous foſſils, might eaſily be miſtaken by this author for the bark of the tree, the ſame appearance being obſerved by Stelluti, in that of Umbria; but it is not ſo eaſy to account for the leaves, which certainly have no connection with a mineral ſubſtance.

John George Liebnecht, in his *Haſſia ſubterranea*, quotes Pillingius's treatiſe on *bitumen* and *lignum foſſile*, for the following deſcription of a ſtratum of this latter kind. He ſays, that at Meiſſlibitz, in the duchy of Altenburg in Saxony, there is a mountain, with a gradual aſcent and fertile ſummit, the

* Quarto, 1724. p. 604.

outward ſtratum of which is vegetable earth, three feet deep; and under it a ſtratum of clay, two fathom thick; then a ſtratum of ſand, made blackiſh by bituminous fumes: *ſubtus lignum putridum, flammam concipiens, pectines annuos referens, ita à naturâ natum, ut nec aqua aquæ, nec lac lacti ſimilius, quam ligno huic minerali lignum vegetabile putridum.*

Our Engliſh naturaliſts have likewiſe deſcribed this foſſil, as found in ſeveral parts of the kingdom, and always in or near ſtrata of clay.

Dr. Plott, in his natural hiſtory of Oxfordſhire *, obſerves, " that the ſcarcity of firing in ſome parts " of that county has induced people to burn a ſort " of black ſubſtance, of a grain ſomewhat like rotten " wood half burnt, partaking alſo of a mineral na- " ture, and therefore by authors called metallophy- " ton, or lignum foſſile. Put into water, it will not " ſwim; and into fire, it conſumes but ſlowly, and " ſends forth very unpleaſant fumes. A vein of it " at Duchlington looked like wood; yet broken, " ſhewed a ſmooth and ſhining ſuperficies, not un- " like to ſtone pitch; and put into the fire, has not " near ſo ill a ſmell. As to the ſubſtance of lignum " foſſile, it is thought to be a cretaceous earth, turned " to what it is by ſubterraneous heats; for that it was " never formerly wood, notwithſtanding its ſpecious " and outward likeneſs, is plain from its never being " found with roots or boughs, or any other ſigns of " wood." And, in his hiſtory of Staffordſhire, he quotes Scoochius's opinion, " that many of the " ſtumps and trunks of trees found in Holland, Zea-

* Page 65.

" land, and Friezland, are of this mineral wood,
" chiefly becauſe moſt of them are found without
" knots or roots."

Dr. Morton, in his natural hiſtory of Northamptonſhire *, mentions two or three varieties of this metallophyton; one of which he deſcribes, as " of
" a dark colour, and having a grain; for in one di-
" rection (which is uſually according to the length
" of the pieces), it cleaves or parts pretty readily
" into plates and ſplinters, the other way it ſnaps
" into ſhorter pieces, and will not cleave at all.
" There is another ſort, which does not ſo readily
" part into flakes. None of theſe are found in any
" large maſſes. They are all, more or leſs, of a
" gloſſy black, and have a denſity or ſmoothneſs within
" like that of bitumen, or jet. In that alſo, they
" reſemble the true bitumen, or piſſaphalton. They
" are not ſo firm and hard as the common coal, and
" are much brittler than canal or jet. They like-
" wiſe all of them agree pretty nearly in the ſame
" properties. In water they ſink. They are all in-
" flammable, but conſume ſlowly in the fire, and
" emit a ſomewhat unpleaſant fume, not unlike that
" of bitumen, only fainter."

Of the ſame kind is the inflammable ſlate, or coal, of Dorſetſhire, found in the cliffs at the north-weſt corner of the iſle of Portland, near the caſtle, of which I have received the following account from a gentleman of that county. The upper ſtratum is the natural black earth 14 feet deep; the next is a greyiſh, ſoft, paving ſtone, 6 inches thick; then follows the vein

* Page 121.

of

of coal, of 10 inches; under which is a black earth, and at the bottom of the clift, clay. It riſes in laminæ, and is diſcovered in the outermoſt part of the clift, whence it is taken, by digging horizontally: ſome is black, and ſome of a rediſh colour; which latter is reputed the worſt, and does not burn ſo well. The aſhes of both are reckoned very good manure for clay ground. It is made uſe of to heat ovens; and when piled up as turff, gives a clear light, and yields a ſtrong bituminous ſmell. It is found more to the weſt, at Chickerel and Fleet, in the ſands, the ſhore being open.

The Kimendge coal, which, in all reſpects, reſembles that before-mentioned, and is ſo called from the place where it is dug, appears in moſt of the cliffs of the iſle of Purbeck, from St. Aldhelm's chapel to Eaſt-Lulworth, and at Oſmington, oppoſite to that part of Portland where the coal is dug. It is found 16 feet below the top of the rock, in great lumps; but the ſtratum does not exceed 2 feet in thickneſs, nor extend far from the ſhore. It emits, when burnt, a ſtrong ſulphureous ſmell, and has a great many yellow ſtrokes and ſpots on it, which ſeem to be marcaſite. It is almoſt wholly uſed by the poor, in their ovens and chimneys, and is ſold for nine pence *per* hogſhead, or ſix ſhillings *per* ton. Under water, and in the cliffs, it is very hard; but when expoſed to the air it ſhivers into pieces like ſlate.

At Kimendge are found ſmall round pieces of this coal, from one to three inches in diameter, one ſide of which is flat, the other tapering, like the lower part of a cone. In ſome there is a ſquare hole in

the center on the flat ſide; in moſt of them two, or ſometimes four holes, near the edge, but they do not penetrate quite through the piece. They are ſometimes found at the top of the cliffs, between two ſtones ſet on edge, and covered with a third, mingled with human bones. Sometimes they are found by themſelves, but always in made ground. The common people call them coal money. I think it can ſcarce be doubted, that they are Britiſh antiquities.

To theſe, I ſhall add Dr. Hoffman's * account of a ſtratum of the ſame kind in Pruſſia, in the neighbourhood of Fiſchauſen, *&c.* where they dig for amber. "The upper ſtratum is ſand, under it a "bed of clay, and then a woody ſtratum, conſiſting "of a ſubſtance like old wood, but inflammable; "under this was a vitriolic mineral; and, laſtly, a "bed of ſand, in which a great quantity of amber was "found." There is a remarkable reſemblance between the order of theſe ſtrata, and thoſe at Bovey; and though no amber has been diſcovered at the latter place, yet it would ſeem, as if there were ſome connection between that and theſe bituminous ſtrata; for it is remarkable, that Theophraſtus †, ſpeaking of an earthy inflammable ſubſtance in Liguria of this kind, adds, that amber is found there.

The reſemblance likewiſe, which the ſtrata of Munden and Allendorf bear to thoſe of Bovey, is remarkable, the ſoil in which they lie conſiſting of clays, boles, and ſand. This is ſo much the caſe at Bovey, that they have large beds of a very fine pipe

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* Obſervationes Phyſico-chemicales, lib. ii. obſ. 23. p. 199.
† De Lapidibus, p. 4.

clay

clay in the neighbourhood, which has for many years been exported to Liverpool, and manufactured there into earthen ware; but there is one now erected at Bovey. The depth of the ſtrata is likewiſe much the ſame; for at Munden they have ſunk 50 feet, without coming to the bottom. The pieces, that form theſe ſtrata, ſeem to lie much in the ſame manner; they are compreſſed, without any intermixture of earth, full of cracks and fiſſures, and lying ſo cloſe upon each other, that the profeſſor ſays, " it could " not be diſcovered, whether they were entire trees, " or only parts of them," and when taken out, they eaſily fell to pieces. The German ſtrata reſemble thoſe of Bovey in colour. That of Allendorf, which Dr. Hollman calls bitumen, and which he diſtinguiſhes from the foſſil wood lying under it, is ſaid to be little inferior in blackneſs, ſplendor, and hardneſs, to jet; and he adds, that the foſſil wood approaches very nearly to the ſame colour. He obſerves, indeed, that the ſtratum of Allendorf is not ſo ſtrongly impregnated with bitumen, and therefore left by the workmen for future ages; and, no doubt, ſome ſpecies of this foſſil have ſo much earth, and ſo little bitumen, that they are not at all, or only in a ſmall degree, inflammable. I ſhall only obſerve further, that the layer of ſtone, which divides the two ſtrata in the quarry of Munden, is a ſtrong objection to the notion of their being foſſil wood, properly ſo called; and that the vein of the ſtratum at Allendorf, being taken in the adit of a mine, could not give an opportunity of examining the nature and diſpoſition of its parts, with the ſame advantage, that we can view the ſtrata at Bovey.

I ſhall

I ſhall conclude this paper with obſerving the ſeveral particulars, in which all the ſpecies of the bituminous foſſils before-mentioned reſemble each other. They ſeem to be generally found between beds of clay, or ſtone; are of a dark brown, or black colour, of a laminated texture: pliable when moiſt, and freſh dug, but criſp and brittle when dry; full of cracks, and eaſily breaking tranſverſely; they all ſink in water, and emit the ſame nauſeous and bituminous ſmell; they differ in being more or leſs ſolid, heavy, and inflammable, according to the proportions and principles of which they conſiſt; and if any doubt could remain of their being a mineral ſubſtance, it muſt be removed by the following analyſis.

One pound of Bovey coal, of the woody kind, powdered, put into a glaſs retort, and diſtilled in ſand, yielded four ounces and a half of phlegm, which had the appearance of common water, but ſomewhat of a bituminous ſmell and taſte; near four ounces of a turbid whitiſh bituminous liquor, of an intolerable fœtid ſmell, and extremely pungent to the tongue; about two drachms of a heavy bituminous matter, which would not mix with the liquor above-mentioned, but ſunk entirely to the bottom, and (which is very remarkable) there was not the leaſt appearance of any light oil floating on the bituminous liquor. There remained in the retort about ſeven ounces of a very black powder, which had the ſame bituminous ſmell, not very heavy; ſome of which being put on a red-hot iron, emitted a little ſmoak, but no flame.

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The aſhes of this foſſil, when burnt, being boiled in water, and the water evaporated, there remained no ſalt behind.

I am, my Lord, *&c.*

Groſvenor-Street, Feb. 28, 1760.

LIV. *A new Method of computing the Sums of certain Series*; *by Mr.* John Landen: *Communicated by Mr.* Thomas Simpſon, *F. R. S.*

Read Feb. 28, 1760.

AS the improving the analytic art, eſpecially any branch of it that relates to the ſummation of ſeries, may, by facilitating computations, conduce to the improvement of ſeveral branches of ſcience; it is preſumed, that this paper, which exhibits a new and eaſy method of computing the ſums of a great number of infinite ſeries, may be acceptable to the mathematical world, and deemed worthy to be inſerted in the Britiſh Philoſophical Tranſactions.

1.

Suppoſing x to be the ſine of the circular arc z, whoſe radius is 1, $\frac{\dot{x}}{\sqrt{1-x^2}}$ will be $= \dot{z}$; and, conſequently, $\frac{\dot{x}}{\sqrt{x^2-1}} = \frac{\dot{z}}{\sqrt{-1}}$. From whence, by taking the correct fluents, we have hyp. log. $\frac{x+\sqrt{x^2-1}}{\sqrt{-1}} = \frac{z}{\sqrt{-1}}$.

Hence,

Hence, writing a for one fourth of the periphery of the circle whoſe radius is 1, and taking x equal to the ſaid radius, we find hyp. log. $\frac{1}{\sqrt{-1}} = \frac{a}{\sqrt{-1}}$; and, conſequently, hyp. log. $\sqrt{-1} = \frac{-a}{\sqrt{-1}}$, and hyp. log. $-1 = \pm\frac{2a}{\sqrt{-1}}$.

2.

The hyp. log. of $\frac{1}{1-x}$ being $= x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4}$, &c.

$\overset{\text{I}}{\text{F}}$ = fluent of $\frac{\dot{x}}{x}$ hyp. log. $\frac{1}{1-x}$, is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2} + \frac{x^4}{4^2}$, &c.

$\overset{\text{II}}{\text{F}}$ = fluent of $\frac{\dot{x}}{x}\overset{\text{I}}{\text{F}} = x + \frac{x^2}{2^3} + \frac{x^3}{3^3} + \frac{x^4}{4^3}$, &c.

$\overset{\text{III}}{\text{F}}$ = fluent of $\frac{\dot{x}}{x}\overset{\text{II}}{\text{F}} = x + \frac{x^2}{2^4} + \frac{x^3}{3^4} + \frac{x^4}{4^4}$, &c.

$\overset{\text{IV}}{\text{F}}$ = fluent of $\frac{\dot{x}}{x}\overset{\text{III}}{\text{F}} = x + \frac{x^2}{2^5} + \frac{x^3}{3^5} + \frac{x^4}{4^5}$, &c.

&c. &c. &c.

3.

By writing, in the firſt equation in the preceding article, $\frac{1}{x}$ inſtead of x, we have

Hyp. log. $\frac{1}{1-\frac{1}{x}} = x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}$, &c.

But the hyp. log. of $\frac{1}{1-\frac{1}{x}}$ is $=$ hyp. log. $\frac{x}{x-1} =$

hyp.

hyp. log. $\frac{1}{1-x}$ + hyp. log. x + hyp. log. $-1 = \pm 2b + X +$ hyp. log. $\frac{1}{1-x}$, b being put for $\frac{a}{\sqrt{-1}}$, and X for the hyp. log. of x.

It is evident, therefore, that

Hyp. log. $\frac{1}{1-x}$ is $= \mp 2b - X + x^{-1} + \frac{x^{-2}}{2} + \frac{x^{-3}}{3}$, &c.

where, of the two ſigns prefixed to $2b$, the upper one takes place, when the hyp. log. of -1 is taken equal to $\frac{-2a}{\sqrt{-1}}$, likewiſe when x is taken equal to $\sqrt{-1}$; and the lower one takes place, when the hyp. log. of -1 is taken equal to $\frac{2a}{\sqrt{-1}}$, alſo when x is taken equal to $\frac{1}{\sqrt{-1}}$: wherefore, if we obſerve to take the value of hyp. log. of -1, as laſt mentioned, and x equal to $\frac{1}{\sqrt{-1}}$, inſtead of $\sqrt{-1}$, we need retain only the lower of the ſaid ſigns.

4.

For brevity ſake, we ſhall, in what follows, put the ſeries $1 + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2}$, &c. $= \overset{II}{P}$,

$1 + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4}$, &c. $= \overset{IV}{P}$,

$1 + \frac{1}{2^6} + \frac{1}{3^6} + \frac{1}{4^6}$, &c. $= \overset{VI}{P}$,

&c. &c.

$1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2}$, &c. $= \overset{II}{Q}$

$$1 - \frac{1}{3^3} + \frac{1}{5^3} - \frac{1}{7^3} +, \&c. = \overset{III}{q},$$

$$1 + \frac{1}{3^4} + \frac{1}{5^4} + \frac{1}{7^4}, \&c. = \overset{IV}{Q}$$

$$1 - \frac{1}{3^5} + \frac{1}{5^5} - \frac{1}{7^5} +, \&c. = \overset{V}{q},$$

$\&c.$ $\&c.$

5.

Multiplying the laſt equation in art. 3. by $\frac{\dot{x}}{x}$, and taking the correct fluents, we have

$$\overset{I}{F} = 2\overset{II}{P} + 2bX - \frac{X^2}{2} - x^{-1} - \frac{x^{-2}}{2^2} - \frac{x^{-3}}{3^2}, \&c.$$

From whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$\overset{II}{F} = 2\overset{II}{P}X + bX^2 - \frac{X^3}{2.3} + x^{-1} + \frac{x^{-2}}{2^3} + \frac{x^{-3}}{3^3}, \&c.$$

Again, multiplying the laſt equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find

$$\overset{III}{F} = 2\overset{IV}{P} + \overset{II}{P}X^2 + \frac{bX^3}{3} - \frac{X^4}{2.3.4} - x^{-1} - \frac{x^{-2}}{2^4} - \frac{x^{-3}}{3^4}, \&c.$$

And, by proceeding in the ſame manner, we find

$$\overset{V}{F} = 2\overset{IV}{P}X + \frac{\overset{II}{P}X^3}{3} + \frac{bX^4}{3.4} - \frac{X^5}{2.3.4.5} + x^{-1} + \frac{x^{-2}}{2^5} + \frac{x^{-3}}{3^5}, \&c.$$

$\&c.$ $\&c.$

6.

Now, it is obvious, that $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, $\&c.$ the value of $\overset{I}{F}$ in art. 2. muſt be equal to $2\overset{II}{P} + 2bX$

$-\frac{X^2}{2}-x^{-1}-\frac{x^{-2}}{2^2}-\frac{x^{-3}}{3^2}$, &c. the value of $\overset{I}{F}$ in art. 5. when both ſeries converge.

Therefore, $\frac{x+x^{-1}}{1^2}+\frac{x^2+x^{-2}}{2^2}+\frac{x^3+x^{-3}}{3^2}$, &c. is then $=2\overset{II}{P}+2bX-\frac{X^2}{2}$.

From which equation, by taking x equal to -1, we have $-\frac{1}{1^2}+\frac{1}{2^2}-\frac{1}{3^2}+\frac{1}{4^2}-$, &c. $=\overset{II}{P}+b^2=\overset{II}{P}-a^2$; and, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have $-\frac{1}{1^2}+\frac{1}{2^2}-\frac{1}{3^2}+\frac{1}{4^2}-$, &c. $=4\overset{II}{P}+3b^2=4\overset{II}{P}-3a^2$.

Therefore $4\overset{II}{P}-3a^2$ is $=\overset{II}{P}-a^2$:

Hence $\overset{II}{P}$ is found $=\frac{2a^2}{3}$.

Moreover $\frac{1}{1^2}+\frac{1}{2^2}+\frac{1}{3^2}+\frac{1}{4^2}$, &c. being $=\overset{II}{P}$, by ſuppoſition; and $-\frac{1}{1^2}+\frac{1}{2^2}-\frac{1}{3^2}+\frac{1}{4^2}-$, &c. $=\overset{II}{P}-a^2$, as found above; we, by ſubtraction, get $\frac{2}{1^2}+\frac{2}{3^2}+\frac{2}{5^2}$, &c. $(=2\overset{II}{Q})=a^2$, and, conſequently $\overset{II}{Q}=\frac{a^2}{2}$.

Scholium.

The hyp. log. of $\frac{1}{1-x}$ being $=x+\frac{x^2}{2}+\frac{x^3}{3}$, &c. we, by writing $1-x$ inſtead of x, have

Hyp. log. of $\frac{1}{x} = 1 - x + \frac{\overline{1-x}^2}{2} + \frac{\overline{1-x}^3}{3}$, &c. and conſequently $X = -1 - x - \frac{\overline{1-x}^2}{2} - \frac{\overline{1-x}^3}{3}$, &c.

Moreover the fluent of $\frac{\dot{x}}{x}$ × hyp. log. of $\frac{1}{1-x}$ is = $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. which vaniſhes when x vaniſhes; and the fluent of $\frac{\dot{x}}{1-x} \times X$ is $= \overline{1-x} + \frac{\overline{1-x}^2}{2^2} + \frac{\overline{1-x}^3}{3^2}$, &c. $- \overset{\text{II}}{P}$, being corrected ſo as to vaniſh when x vaniſhes.

But the fluent of $\frac{\dot{x}}{x}$ × hyp. log. of $\frac{1}{1-x}$ + fluent of $\frac{\dot{x}}{1-x} \times X$ is = X × hyp. log. of $\frac{1}{1-x}$, which alſo vaniſhes when x vaniſhes.

Therefore X × hyp. log. of $\frac{1}{1-x}$ is $= x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. + $1 - x + \frac{\overline{1-x}^2}{2^2} + \frac{\overline{1-x}^3}{3^2}$, &c. $- \overset{\text{II}}{P}$.

From whence, by taking x equal to $\frac{1}{2}$, we find ſquare of hyp. log. of $2 = 2 \times \frac{1}{1^2.2^1} + \frac{1}{2^2.2^2} + \frac{1}{3^2.2^3}$, &c. $- \overset{\text{II}}{P}$: hence, $\overset{\text{II}}{P}$ being before found $= \frac{2a^2}{3}$, it appears that, when x is $= \frac{1}{2}$, the ſeries $x + \frac{x^2}{2^2} + \frac{x^3}{3^2}$, &c. is $= \frac{a^2}{3} - \frac{1}{2} \times \overline{\text{hyp. log. of } 2}^2$.

7.

Furthermore, $x + \frac{x^2}{2^3} + \frac{x^3}{3^3}$, &c. the value of $\overset{\text{III}}{F}$ in art. 2. muſt be equal to $2\overset{\text{II}}{P}X + bX^2 - \frac{X^3}{2.3} + x^{-1}$

+

$+\frac{x^{-2}}{2^3}+\frac{x^{-3}}{3^3}$, &c. the value of $\overset{II}{F}$ in art. 5. when both ſeries converge.

Therefore $\frac{x-x^{-1}}{1^3}+\frac{x^2-x^{-2}}{2^3}+\frac{x^3-x^{-3}}{3^3}$, &c. is then $= 2\overset{II}{P}X + bX^2 - \frac{X^3}{2.3}$.

From whence, by taking x equal to -1, we have $4b\overset{II}{P} + 4b^3 - \frac{8b^3}{2.3} = 0$; and, conſequently, $\overset{II}{P} = \frac{2a^2}{3}$, as before found.

And, by taking x equal to $\frac{1}{\sqrt{-1}}$, we find

$$\frac{2}{\sqrt{-1}} \times \overset{III}{q} = 2b\overset{II}{P} + b^3 - \frac{b^3}{2.3} = \frac{4a^3}{3\sqrt{-1}} - \frac{a^3}{\sqrt{-1}} + \frac{a^3}{2.3\sqrt{-1}} = \frac{a^3}{2\sqrt{-1}}.$$

Therefore $\overset{III}{q}$ is $= \frac{a^3}{4}$.

8.

From what is done above, it evidently follows, that

$$-\overset{IV}{P} \text{ is} = \frac{2b^2\overset{II}{P}}{3} + \frac{2.8\,b^4}{3.4.5},$$

$$-\overset{VI}{P} = \frac{2b^2\overset{IV}{P}}{3} + \frac{8b^4\overset{II}{P}}{3.4.5} + \frac{3.32\,b^6}{3.4.5.6.7},$$

&c. &c.

$$-\overset{IV}{Q} = b^2\overset{II}{P} + \frac{3.2\,b^4}{3.4},$$

$$-\overset{VI}{Q} = b^2\overset{IV}{P} + \frac{4b^4\overset{II}{P}}{3.4} + \frac{5.8\,b^6}{3.4.5.6},$$

&c. &c.

$\frac{\overset{v}{q}}{\sqrt{-1}}$ is $= b\overset{v}{P} + \frac{b^3\overset{II}{P}}{2.3} + \frac{9b^5}{2.2.3.4.5}$,

$\frac{\overset{VII}{q}}{\sqrt{-1}} = b\overset{VI}{P} + \frac{b^3\overset{IV}{P}}{2.3} + \frac{b^5\overset{II}{P}}{2.3.4.5} + \frac{13b^7}{2.2.3.4.5.6.7}$,

&c. &c.

From whence the values of $\overset{IV}{P}$, $\overset{VI}{P}$, &c. $\overset{IV}{Q}$, $\overset{VI}{Q}$, &c. $\overset{v}{q}$, $\overset{VII}{q}$, &c. may be eaſily obtained, in terms of a.

9.

Hyp. log. $\left|\frac{1+x}{1-x}\right|^{\frac{1}{2}}$ being $= x + \frac{x^3}{3} + \frac{x^5}{5}$, &c.

$\overset{I}{G}$ = fluent of $\frac{\dot{x}}{x}$ hyp. log. $\left|\frac{1+x}{1-x}\right|^{\frac{1}{2}}$ is $= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c.

$\overset{II}{G}$ = fluent of $\frac{\dot{x}}{x}\overset{I}{G} = x + \frac{x^3}{3^3} + \frac{x^5}{5^3}$, &c.

$\overset{III}{G}$ = fluent of $\frac{\dot{x}}{x}\overset{II}{G} = x + \frac{x^3}{3^4} + \frac{x^5}{5^4}$, &c.

&c. &c. &c.

10.

By writing, in the firſt equation in the preceding article, $\frac{1}{x}$ inſtead of x, we have

Hyp. log. $\left|\frac{1+\frac{1}{x}}{1-\frac{1}{x}}\right|^{\frac{1}{2}} = x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}$, &c.

But the hyp. log. of $\left|\frac{1+\frac{1}{x}}{1-\frac{1}{x}}\right|^{\frac{1}{2}}$ is $=$ hyp. log. $\left|\frac{x+1}{x-1}\right|^{\frac{1}{2}}$

$=$ hyp. log. $\left|\frac{1+x}{1-x}\right|^{\frac{1}{2}} \mp$ hyp. log. $\sqrt{-1} = \mp b +$ hyp. log. $\left|\frac{1+x}{1-x}\right|^{\frac{1}{2}}$.

It

It is manifeſt, therefore, that

Hyp. log. $\overline{\frac{1+x}{1-x}}^{\frac{1}{2}}$ is $= \mp b + x^{-1} + \frac{x^{-3}}{3} + \frac{x^{-5}}{5}$, &c.

where, with reſpect to the two ſigns prefixed to b, the ſame obſervation may be made as in art. 3.

11.

Multiplying the laſt equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we have

$$\overset{\text{I}}{G} = 2\overset{\text{II}}{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}, \text{ \&c.}$$

From whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the fluents, we get

$$\overset{\text{II}}{G} = 2\overset{\text{II}}{Q}X + \frac{bX^2}{2} + x^{-1} + \frac{x^{-3}}{3^3} + \frac{x^{-5}}{5^3}, \text{ \&c.}$$

Again, multiplying the laſt equation by $\frac{\dot{x}}{x}$, and taking the correct fluents, we find

$$\overset{\text{III}}{G} = 2\overset{\text{IV}}{Q} + \overset{\text{II}}{Q}X^2 + \frac{bX^3}{2.3} - x^{-1} - \frac{x^{-3}}{3^4} - \frac{x^{-5}}{5^4}, \text{ \&c.}$$

And, by proceeding in the ſame manner, we find

$$\overset{\text{IV}}{G} = 2\overset{\text{IV}}{Q}X + \frac{\overset{\text{II}}{Q}X^3}{3} + \frac{bX^4}{2.3.4} + x^{-1} + \frac{x^{-3}}{3^5} + \frac{x^{-5}}{5^5}, \text{ \&c.}$$

&c. &c.

12.

Now, it is obvious, that $x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c. the value of $\overset{\text{I}}{G}$ in art. 9. muſt be equal to $2\overset{\text{II}}{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, &c. the value of $\overset{\text{I}}{G}$ in art. 11. when both ſeries converge.

There-

Therefore $\frac{x+x^{-1}}{1^2}+\frac{x^3+x^{-3}}{3^2}+\frac{x^5+x^{-5}}{5^2}$, &c. is then $= 2\overset{II}{Q}+bX$.

From whence, by taking x equal to $\frac{1}{\sqrt{-1}}$, we have $2\overset{II}{Q}+b^2=0$; and, consequently, $\overset{II}{Q}=\frac{a^2}{2}$, as in art. 6.

13.

Likewise $x+\frac{x^3}{3^3}+\frac{x^5}{5^3}$, &c. the value of $\overset{II}{G}$ in art. 9. must be equal to $2\overset{II}{Q}X+\frac{bX^2}{2}+x^{-1}+\frac{x^{-3}}{3^3}+\frac{x^{-5}}{5^3}$, &c. the value of $\overset{II}{G}$ in art. 11. when both series converge.

Therefore $\frac{x-x^{-1}}{1^3}+\frac{x^3-x^{-3}}{3^3}+\frac{x^5-x^{-5}}{5^3}$, &c. is then $= 2\overset{II}{Q}X+\frac{bX^2}{2}$.

Hence, by taking $x=\frac{1}{\sqrt{-1}}$, we find $\frac{2}{\sqrt{-1}}\times\overset{III}{q}$ $=2b\overset{II}{Q}+\frac{b^3}{2}=\frac{a^3}{2\sqrt{-1}}$; and, consequently, $\overset{III}{q}=\frac{a^3}{4}$, as in art. 7.

14.

From what is done in the last five articles, it evidently follows, that

$$-\overset{IV}{Q} \text{ is } =\frac{b^2\overset{II}{Q}}{2}+\frac{b^4}{2.2.3},$$

$$\frac{\overset{V}{q}}{\sqrt{-1}}=b\overset{IV}{Q}+\frac{b^3\overset{II}{Q}}{2.3}+\frac{b^5}{2.2.3.4},$$

$$-\overset{VI}{Q} \text{ is} = \frac{b^2\overset{IV}{Q}}{2} + \frac{b^4\overset{II}{Q}}{2.3.4} + \frac{b^6}{2.2.3.4.5},$$

$$\frac{\overset{VII}{q}}{\sqrt{-1}} = b\overset{VI}{Q} + \frac{b^3\overset{IV}{Q}}{2.3} + \frac{b^5\overset{II}{Q}}{2.3.4.5} + \frac{b^7}{2.2.3.4.5.6},$$

&c. &c.

From whence (as well as from the theorems in art. 8.) may the values of $\overset{IV}{Q}$, $\overset{V}{q}$, $\overset{VI}{Q}$, $\overset{VII}{q}$, &c. be readily found, in terms of a.

15.

$\overset{I}{G}$ being $= x + \frac{x^3}{3^2} + \frac{x^5}{5^2}$, &c. by art. 9.

$\overset{I}{H}$ = fluent of $x\,\dot{x}\overset{I}{G}$ is $= \frac{x^3}{1^2.3} + \frac{x^5}{3^2.5} + \frac{x^7}{5^2.7}$, &c.

$\overset{II}{H}$ = fluent of $\frac{\dot{x}}{x}\overset{I}{H} = \frac{x^3}{1^2.3^2} + \frac{x^5}{3^2.5^2} + \frac{x^7}{5^2.7^2}$, &c.

$\overset{III}{H}$ = fluent of $x\,\dot{x}\overset{II}{H} = \frac{x^5}{1^2.3^2.5} + \frac{x^7}{3^2.5^2.7} + \frac{x^9}{5^2.7^2.9}$, &c.

$\overset{IV}{H}$ = fluent of $\frac{\dot{x}}{x}\overset{III}{H} = \frac{x^5}{1^2.3^2.5^2} + \frac{x^7}{3^2.5^2.7^2} + \frac{x^9}{5^2.7^2.9^2}$, &c.

&c. &c. &c.

16.

Moreover, $\overset{I}{G}$ being $= 2\overset{II}{Q} + bX - x^{-1} - \frac{x^{-3}}{3^2} - \frac{x^{-5}}{5^2}$, &c. by art. 11. we, by multiplying by $x\,\dot{x}$, and taking the correct fluents, get $\overset{I}{H} = x^2\overset{II}{Q} - \overset{II}{Q} + \frac{b\,x^2\,X}{2} - \frac{b\,x^2}{4} + \frac{b}{4} - x + 1 + 2\overset{II}{S} + \frac{x^{-1}}{1.3^2} + \frac{x^{-3}}{3.5^2} + \frac{x^{-5}}{5.7^2}$, &c. $\overset{II}{S}$ being put for the series $\frac{1}{1^2.3^2} + \frac{1}{3^2.5^2} + \frac{1}{5^2.7^2}$, &c.

Now, it is obvious, that this value of $\overset{\text{II}}{\dot{H}}$ must be equal to the value of $\overset{\text{I}}{\dot{H}}$ in the preceding article, when both series converge.

Therefore $\frac{3x^3 - x^{-1}}{1^2 \cdot 3^2} + \frac{5x^5 - 3x^{-3}}{3^2 \cdot 5^2} + \frac{7x^7 - 5x^{-5}}{5^2 \cdot 7^2}$, &c. is then $= x^2 \overset{\text{II}}{Q} - \overset{\text{II}}{Q} + \frac{bx^2 X}{2} - \frac{bx^2}{4} + \frac{b}{4} - x + 1 + 2\overset{\text{II}}{S}$.

Hence, by taking x equal to -1, we find $-2\overset{\text{II}}{S} = b^2 + 2 + 2\overset{\text{II}}{S}$; and, consequently, $\overset{\text{II}}{S} = \frac{a^2}{4} - \frac{1}{2}$.

And, by taking x equal to $\frac{1}{\sqrt{-1}}$, we find

$$-\frac{2}{\sqrt{-1}} \times \frac{1}{1^2 \cdot 3^2} - \frac{1}{3^2 \cdot 5^2} + \frac{1}{5^2 \cdot 7^2} -, \text{ \&c.} = -2\overset{\text{II}}{Q}$$

$$-\frac{b^2}{2} + \frac{b}{2} - \frac{1}{\sqrt{-1}} + 1 + 2\overset{\text{II}}{S} = \frac{a}{2\sqrt{-1}} - \frac{1}{\sqrt{-1}};$$

and, consequently, $\frac{1}{1^2 \cdot 3^2} - \frac{1}{3^2 \cdot 5^2} + \frac{1}{5^2 \cdot 7^2} -$, &c. $= \frac{1}{2} - \frac{a}{4}$.

17.

Seeing that $\overset{\text{II}}{Q}$ is $= \frac{a^2}{2}$, and $\overset{\text{II}}{S} = \frac{a^2}{4} - \frac{1}{2}$, it follows, from the last article, that

$\overset{\text{I}}{H}$ is $= x^2 \overset{\text{II}}{Q} + \frac{bx^2 X}{2} - \frac{bX^2}{4} + \frac{b}{4} - x + \frac{x^{-1}}{1 \cdot 3^2} + \frac{x^{-3}}{3 \cdot 5^2} + \frac{x^{-5}}{5 \cdot 7^2}$, &c.

From whence, by multiplying by $\frac{\dot{x}}{x}$, and taking the correct fluent, we get

$\overset{\text{II}}{H}$

$$\overset{II}{H} = \frac{x^2\overset{II}{Q}}{2} + \frac{bx^2X}{4} - \frac{bx^2}{4} + \frac{b}{4} + \frac{bX}{4} - x + \frac{a^2}{4} - \frac{x^{-1}}{1^2\cdot 3^2} - \frac{x^{-3}}{3^2\cdot 5^2} - \frac{x^{-5}}{5^2\cdot 7^2}, \&c.$$

And hence, by multiplying by $x\dot{x}$, and taking the correct fluents, we have

$$\overset{III}{H} = \frac{x^4\overset{II}{Q}}{8} + \frac{bx^4X}{16} - \frac{5bx^4}{64} + \frac{bx^2}{16} + \frac{bx^2X}{8} + \frac{b}{64} - \frac{x^3}{3} - \frac{x}{9} + \frac{4}{9} + \frac{a^2x^2}{8} - \frac{3a^2}{16} + 4\overset{III}{S} + \frac{x^{-1}}{1\cdot 3^2\cdot 5^2} + \frac{x^{-3}}{3\cdot 5^2\cdot 7^2} + \frac{x^{-5}}{5\cdot 7^2\cdot 9^2}, \&c.$$ $\overset{III}{S}$ being put for the series

$$\frac{1}{1^2\cdot 3^2\cdot 5^2} + \frac{1}{3^2\cdot 5^2\cdot 7^2} + \frac{1}{5^2\cdot 7^2\cdot 9^2}, \&c.$$

Now, this value of $\overset{III}{H}$ being equal to the value of $\overset{III}{H}$ in art. 15. when both ſeries converge, it follows, that $\frac{5x^5 - x^{-1}}{1^2\cdot 3^2\cdot 5^2} + \frac{7x^7 - 3x^{-3}}{3^2\cdot 5^2\cdot 7^2} + \frac{9x^9 - 5x^{-5}}{5^2\cdot 7^2\cdot 9^2}, \&c.$ is then

$$= \frac{x^4\overset{II}{Q}}{8} + \frac{bx^4X}{16} - \frac{5bx^4}{64} + \frac{bx^2}{16} + \frac{bx^2X}{8} + \frac{b}{64} - \frac{x^3}{3} - \frac{x}{9} + \frac{4}{9} + \frac{a^2x^2}{8} + \frac{3a^2}{16} - 4\overset{III}{S}.$$

Hence, by taking x equal to -1, we find $-4\overset{III}{S} = \frac{3b^2}{8} + \frac{8}{9} + 4\overset{III}{S}$; and, conſequently, $\overset{III}{S} = \frac{3a^2}{64} - \frac{8}{9}$.

Many other inſtances of the uſe of this method might be given; but theſe may ſuffice to enable the intelligent reader to purſue the ſpeculation farther, at his pleaſure.

LV. *Conjectures concerning the Cauſe, and Obſervations upon the Phænomena of Earthquakes; particularly of that great Earthquake of the Firſt of* November, 1755, *which proved ſo fatal to the City of* Liſbon, *and whoſe Effects were felt as far as* Africa, *and more or leſs throughout almoſt all* Europe; *by the Reverend* John Michell, *M. A. Fellow of* Queen's *College*, Cambridge.

INTRODUCTION.

Read Feb. 28. March 6. 13. 20. 27. 1760.

ART. 1. IT has been the general opinion of philoſophers, that earthquakes owe their origin to ſome ſudden exploſion in the internal parts of the earth. This opinion is very agreeable to the phænomena, which ſeem plainly to point out ſomething of that kind. The conjectures, however, concerning the cauſe of ſuch an exploſion, have not been yet, I think, ſufficiently ſupported by facts; nor have the more particular effects, which will ariſe from it, been traced out; and the connexion of them with the phænomena explained. To do this, is the intent of the following pages; and this we are now the better enabled to do, as the late dreadful earthquake of the 1ſt of November 1755 ſupplies us with more * facts, and

* Many of theſe facts are collected together in the 49th volume of the Philoſophical Tranſactions. The ſame are alſo to be found, with

and thoſe better related, than any other earthquake of which we have an account.

2. That theſe concuſſions ſhould owe their origin to ſomething in the air, as it has ſometimes been imagined, ſeems very ill to correſpond with the phænomena. This, I apprehend, will ſufficiently appear, as thoſe phænomena are hereafter recounted; nor does there appear to be any ſuch certain and regular connexion between earthquakes and the ſtate of the air, when they happen, as is ſuppoſed by thoſe who hold this opinion. It is ſaid, for inſtance, that earthquakes always happen in calm ſtill weather: but that this is not always ſo, may be ſeen in an account of the * earthquakes in Sicily of 1693, where we are told, "the ſouth winds have blown very much, which "ſtill have been impetuous in the moſt ſenſible "earthquakes, and the like has happened at other "times."

3. Other examples to the ſame purpoſe we have in an account of the earthquakes that happened in New England in 1727 and 1728; the author of

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with ſome additional ones, in "The Hiſtory and Philoſophy of "Earthquakes," (a work well worth the peruſal of thoſe, who are deſirous of being acquainted with this ſubject). The author of it has given us, beſides the aforeſaid facts, a very judicious abridgment of ten of the moſt conſiderable writers upon the ſubject. I have taken the greateſt part of my authorities either from this author, or the Philoſophical Tranſactions, that thoſe who would wiſh to examine them, may have an opportunity of doing it the more eaſily; ſome things only, which were not to be met with in theſe, and which yet were neceſſary to my purpoſe, I have been obliged to ſeek for elſewhere.

* See Philoſ. Tranſ. N° 207. or vol. ii. p. 408. Lowthorp's Abridgment.

which

which ſays, that he could neither obſerve any connexion between the weather and the earthquakes, nor any prognoſtic of them; for that they happened alike in all kinds of weather, at all times of the tides, and at all times of the moon *.

4. If, however, it ſhould ſtill be ſuppoſed, notwithſtanding theſe inſtances to the contrary, that there is ſome general connexion between earthquakes and the weather, at the time when they happen, yet, ſurely, it is far more probable, that the air ſhould be affected by the cauſes of earthquakes, than that the earth ſhould be affected in ſo extraordinary a manner, and to ſo great a depth; and that this,

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* See Philoſ. Tranſ. N° 409. or vol. vi. part ii. p. 202. Eames's Abridgment.—To theſe authorities, we may add the opinion of Monſ. Bertrand, who expreſſes himſelf, upon this occaſion, in the following manner. "Ariſtotle, Pliny, and "Seneca, tell us, that earthquakes are preceded by a calm and "ſerene air. This is, indeed, often the caſe, but not always. "I don't know, upon an examination of the whole, if there are "not as many exceptions to this rule, as examples that confirm "it. Some authors again have thought, that they might look on "a dark ſky, lightenings, and ſudden ſtorms, as the forerunners "of earthquakes." Then relating ſome inſtances of ſhocks that happened in calm and ſerene weather, he adds, "On the other "hand, it appears, from the examples, which we have before "related, that many earthquakes have happened at the time of "great rains, violent winds, and with a cloudy ſky; ſo that one "cannot find any certain prognoſtic of them in the ſtate of the "atmoſphere." See *Memoires Hiſtoriques et Phyſiques ſur les tremblemens de Terre, par Monſ. Bertrand, a la Haye* 1757. This author, in theſe ſenſible memoirs, has obliged the public with a circumſtantial account of all the facts he could collect, relating to the earthquakes of Switzerland, or thoſe of other places, that ſeemed to be connected with them. The whole ſeems to be done with care and fidelity, and without the leaſt attachment to any particular ſyſtem.

and

and all the other circumſtances attending theſe motions, ſhould be owing to ſome cauſe reſiding in the air.

5. Let us then, rejecting this hypotheſis, ſuppoſe, that earthquakes have their origin under ground, and we need not go far in ſearch of a cauſe, whoſe real exiſtence in nature we have certain evidence of, and which is capable of producing all the appearances of theſe extraordinary motions. The cauſe I mean is ſubterraneous fires. Theſe fires, if a large quantity of water ſhould be let out upon them ſuddenly, may produce a vapour, whoſe quantity and elaſtic force may be fully ſufficient for that purpoſe. The principal facts, from which I would prove, that theſe fires are the real cauſe of earthquakes, are as follow.

SECTION I.

6. *Firſt*, The ſame places are ſubject to returns of earthquakes, not only at ſmall intervals for ſome time after any conſiderable one has happened, but alſo at greater intervals of ſome ages.

7. Both theſe facts ſufficiently appear, from the accounts we have of earthquakes. The tremblings and ſhocks of the earth at * Jamaica in 1692, at * Sicily in 1693, and at * Liſbon in 1755, were repeated ſometimes at larger, and ſometimes at ſmaller intervals, for ſeveral months. The ſame thing has been obſerved in all other very violent earthquakes. At † Lima, from the 28th October 1746, to the

* See the accounts of theſe in the Philoſ. Tranſ.

† See Antonio d'Ulloa's Voyage to Peru, part ii. book i. ch. 7.

24th February 1747 (the time when the account of them was ſent from thence), there had been numbered no leſs than 451 ſhocks, many of them little inferior to the firſt great one, which deſtroyed that city.

8. The returns of earthquakes alſo, in the ſame places, at larger diſtances of time, are confirmed by all hiſtory. Conſtantinople, and many parts of Aſia Minor, have ſuffered by them, in many different ages: Sicily has been ſubject to them, as far back as the remains even of fabulous hiſtory can inform us of: Liſbon did not feel the effects of them for the firſt time in 1755: Jamaica has frequently been troubled with them, ſince the Engliſh firſt ſettled there; and the Spaniards, who were there before, uſed to build their houſes of wood, and only one ſtory high, for fear of them: * Lima, Callao, and the parts adjacent, were almoſt totally deſtroyed by them twice, within the compaſs of about ſixty years, ſcarce any building being left ſtanding, and the latter being both times overflowed by the ſea: nor were theſe the only inſtances of the like kind, which have happened there; for, from the year 1582 to 1746, they have had no leſs than ſixteen very violent earthquakes, beſides an infinity of leſs conſiderable ones; and the Spaniards, at their firſt ſettling there, were told by the old inhabitants, when they ſaw them building high houſes, that they were building their own ſepulchres †.

9. *Secondly*,

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* See the place above-quoted.

† What is here ſaid, is taken from d'Ulloa's Voyage to Peru, the Hiſtory and Philoſophy of Earthquakes, the Philoſ. Tranſ. *&c.*

where

9. *Secondly*, Thoſe places that are in the neighbourhood of burning mountains, are always ſubject to frequent earthquakes; and the eruptions of thoſe mountains, when violent, are generally attended with them.

10. Aſia Minor and Conſtantinople may be looked upon as in the neighbourhood of Santerini. The countries alſo about * Ætna, Veſuvius, mount Hæcla, *&c.* afford us ſufficient proofs to the ſame purpoſe. But, of all the places in the known world, I ſuppoſe, no countries are ſo ſubject to earthquakes, as † Peru, Chili, and all the weſtern parts of South America; nor is there any country in the known world ſo full of volcanos: for, throughout all that long range of mountains, known by the name of the Andes, from 45 degrees ſouth latitude, to ſeveral degrees north of the line, as alſo throughout all Mexico, being about 5000 miles in extent, there is a continued chain of them ‡.

11. *Thirdly*, The motion of the earth in earthquakes is partly tremulous, and partly propagated by waves, which ſucceed one another ſometimes at larger and ſometimes at ſmaller

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where many more examples, to the ſame purpoſe, are to be met with. See alſo *Memoires ſur les tremblemens de Terre*; in which are mentioned above 130 repetitions of earthquakes, that have happened, within the compaſs of 960 years, in Switzerland.

* See many inſtances of this in vol. ii. of Lowthorp's Abr. of the Philoſ. Tranſ.

† Monſ. Bouguer ſays, that ſcarce a week paſſes without earthquakes in ſome part of Peru. See Hiſt. of Earthq. p. 205.

‡ See the Maps of theſe countries, Condamine's Voyage down the Maranon, Acoſta's Nat. Hiſt. of the Indies, *&c.*

diſtances; and this latter motion is generally propagated much farther than the former.

12. The former part of this propoſition wants no confirmation: for the proof of the latter, *viz.* the wave-like motion of the earth, we may appeal to many accounts of earthquakes: it was very remarkable in the two, which happened at Jamaica in * 1687-8 and * 1692. In an account of the former, it is ſaid, that a gentleman there ſaw the ground riſe like the ſea in a wave, as the earthquake paſſed along, and that he could diſtinguiſh the effects of it, to ſome miles diſtance, by the motion of the tops of the trees on the hills. Again, in an account of the latter, it is ſaid, " the ground heaved and ſwelled, " like a rolling ſwelling ſea," inſomuch, that people could hardly ſtand upon their legs by reaſon of it.

13. The ſame has been obſerved in the earthquakes of † New England, where it has been very remarkable. A gentleman giving an account of one, that happened there the 18th November 1755, ſays, the earth roſe in a wave, which made the tops of the trees vibrate ten feet, and that he was forced to ſupport himſelf, to avoid falling, whilſt it was paſſing.

14. The ſame alſo was obſerved at ‡ Liſbon, in the earthquake of the 1ſt November 1755, as may be

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* See Phil. Tranſ. N° 209. or vol. ii. Lowthorp's Abridgment, p. 410.

† See Philoſ. Tranſ. vol. l. p. 1, *&c.*

‡ See the accounts collected together, in the 49th volume of the Philoſ. Tranſ. or in Hiſt. and Philoſ. of Earthq. and particularly p. 315. where it is ſaid, " A moſt dreadful earthquake ſhook by " ſhort,

be plainly collected from many of the accounts that have been publiſhed concerning it, ſome of which affirm it expreſly: and this wave-like motion was propagated to far greater diſtances than the other tremulous one, being perceived by the motion of waters, and the hanging branches in churches, through all Germany, amongſt the Alps, in Denmark, Sweden, Norway, and all over the Britiſh iſles.

15. *Fourthly*, It is obſerved in places, which are ſubject to frequent earthquakes, that they generally come to one and the ſame place, from the ſame point of the compaſs. I may add alſo, that the velocity, with which they proceed, (as far as one can collect it from the accounts of them) is the ſame; but the velocity of the earthquakes of different countries is very different.

16. Thus all the ſhocks, that ſucceeded the firſt great one at Liſbon in 1755, as well as the firſt itſelf, came from the * north-weſt. This is aſſerted by the perſon, who ſays, he was about writing a hiſtory of the earthquakes there: all the other accounts alſo confirm the ſame thing; for what ſome ſay, that they came from the north, and others, that they came

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" ſhort, but quick vibrations, the foundations of all Liſbon;
" then, with a ſcarcely perceptible pauſe, the nature of the mo-
" tion changed, and every building was toſſed like a waggon
" driven violently over rough ſtones, which laid in ruins almoſt
" every houſe, church, *&c.*"

For the wave-like motion at Oporto, ſee Phil. Tranſ. vol. xlix. p. 418. for the ſame at Gibraltar, ſee Hiſt. and Philoſ. of Earthq. p. 322.

* See Philoſ. Tranſ. vol. xlix. p. 410.

from the weſt, cannot be looked on as any reaſonable objection to this, but rather the contrary. The velocity alſo, with which they were all propagated, was the ſame, being at leaſt equal to that of ſound; for they all followed * immediately after the noiſe that preceded them, or rather the noiſe and the earthquake came together: and this velocity agrees very well with the intervals between the time when the firſt ſhock was felt at Liſbon, and the time when it was felt at other diſtant places, from the compariſon of which, it ſeems to have travelled at the rate of more than † twenty miles *per* minute.

17. An hiſtorical account of the earthquakes, which have happened in ‡ New England, ſays, that, of five conſiderable ones, three are known to have come from the ſame point of the compaſs, *viz.* the north-weſt: it is uncertain from what point the other two came, but it is ſuppoſed that they came from the ſame with the former. The ‖ velocity of theſe has been much leſs than that of the Liſbon earthquakes: this appears from the interval between the preceding noiſe, and the ſhock, as well as from the wave-like motion before-mentioned.

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* See Philoſ. Tranſ. vol. xlix. p. 414. or Hiſt. and Philoſ. of Earthq. p. 315.

† See Art. 97.

‡ See Philoſ. Tranſ. vol. l. p. 9.

‖ As in ſome earthquakes the velocity, with which they are propagated, is much leſs than in others, it is evident, that they can by no means be owing to any cauſe reſiding in the air: for any ſhock communicated to the air, muſt neceſſarily move with a velocity neither greater nor leſs than that of ſounds; that is, at the rate of about thirteen miles *per* minute.

18. All

18. All the greater earthquakes, that have been felt at * Jamaica, ſeem, by the accounts given of them, to have come from the ſea, and, paſſing by Port-Royal, to have gone northwards. The velocity of theſe alſo was far ſhort of the velocity of the Liſbon earthquakes.

19. The earthquake of † London, on the 8th of March 1750, was ſuppoſed to move from eaſt to weſt. I have been credibly informed, that the ſame thing happened in a ſlight ſhock, which was felt there in the laſt century, as the perſon, who told me this, had an opportunity of obſerving; for being, by accident, in a ſcalemaker's ſhop at the time when it happened, he found that all the ſcales vibrated from eaſt to weſt.

20. All the ſhocks that have been lately felt at Brigue in Valais, have likewiſe come from the ſame point of the compaſs, *viz.* the ſouth ‡.

21. *Fifthly*, The great Liſbon earthquake has been ſucceeded by ſeveral local ones ſince, the extent of which has been much leſs.

22. Such were the earthquakes in Switzerland; thoſe on the borders of France and Germany; thoſe in Barbary, *&c.* ‖

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* See the accounts of them in Philoſ. Tranſ. N° 209. or vol. ii. Lowthorp's Abr. p. 410, *&c.*

† See Hiſt. and Philoſ. of Earthq. p. 250. or Philoſ. Tranſ. vol. x. Martyn's Abr. Meteorology, paſſim.

‡ See Philoſ. Tranſ. vol. xlix. p. 620. The ſame has been obſerved at Smyrna alſo, ſee Philoſ. Tranſ. N° 495, or Martyn's Abr. vol. x. p. 526.

‖ See the accounts of theſe collected together in Philoſ. Tranſ. vol. xlix. or in the Hiſt. and Philoſ. of Earthq.

Sect. II.

23. How well ſoever theſe facts may agree with the ſuppoſition before laid down, That ſubterraneous fires are the cauſe of earthquakes, one doubt, however, may perhaps remain; *viz.* how it is poſſible that fires ſhould ſubſiſt, which have no communication with the outward air? In anſwer to this, I might alledge the example of green plants, which take fire by fermentation, when laid together in heaps; where the admiſſion of the outward air is ſo far from being neceſſary, that it will effectually prevent their doing ſo. But, to paſs by this, we have many inſtances more immediately to the purpoſe.

24. It can hardly be ſuppoſed, that the fires of the generality of volcanos receive any ſupply of freſh air (for this muſt effectually be prevented by that vapour, which is continually ruſhing out at all their vents), and yet they ſubſiſt, and frequently even increaſe, for many ages. Now, theſe are fires of the very ſame kind with thoſe, which I ſuppoſe to be the cauſe of earthquakes. Other facts, ſtill more expreſly to the purpoſe, are as follow:

25. In the earthquake of the 1ſt of November 1755, we are told, that both ſmoke and light flames were ſeen on the coaſt of Portugal, near Colares; and that, upon occaſion of ſome of the ſucceeding ſhocks, a ſlight ſmell of ſulphur was perceived to accompany a "fog, which came from the ſea, from the ſame "quarter, whence the ſmoke appeared *."

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* See Philoſ. Tranſ. vol. xlix. p. 414, &c.

26. In

26. In an account of an earthquake in New England, it is ſaid, that at Newbury, forty miles from Boſton, the earth opened, and threw up ſeveral cart-loads of ſand and aſhes; and that the ſand was alſo ſlightly impregnated with ſulphur, emitting a blue flame, when laid on burning coals *.

27. One of the relaters of the earthquake in Jamaica in 1692, has theſe words: " In Port-Royal, " and in many places all over the iſland, much ſul- " phureous combuſtible matter hath been found " (ſuppoſed to have been thrown out upon the " opening of the earth), which, upon the firſt touch " of fire, would flame and burn like a candle.

28. " St. Chriſtopher's was heretofore much " troubled with earthquakes, which, upon the erup- " tion there of a great mountain of combuſtible mat- " ter, which ſtill continues, wholly ceaſed, and have " never been felt there ſince †."

29. Again, we are told, that, on the 20th November 1720, a burning ‡ iſland was raiſed out of the ſea, near Tercera, one of the Azores, at which place, ſeveral houſes were ſhaken down by an earthquake, which attended the eruption of it. This iſland was about three leagues in diameter, and nearly round; from whence it is manifeſt, that the quantity of pumice ſtones and melted matter, which muſt have been requiſite to form it, was amazingly great:

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* See Philoſ. Tranſ. N° 409. or vol. vi. part ii. p. 201. Eames's Abr.

† See Philoſ. Tranſ. N° 209. or vol. ii. p. 418. Lowthorp's Abr.

‡ See Philoſ. Tranſ. N° 372. or vol. vi. part ii. p. 203. Eames's Abr.

in

in all probability, it muſt have far exceeded all that has been thrown out of Ætna and Veſuvius together within the laſt two thouſand years. This may ſerve to ſatisfy us, that the fire which occaſioned all this, muſt have ſubſiſted for many years, not to ſay ages, and this without any communication with the external air. It is worth obſerving, that * ſeveral inſtances of this kind have happened amongſt the Azores. There are beſides many marks of ſubterraneous fires about theſe iſlands, ſeveral places ſending up ſmoke or flames. Theſe iſlands are alſo ſubject to violent and frequent earthquakes.

30. We have more inſtances to the ſame purpoſe, near the iſland of Santerini in the Archipelago, where there have been ſeveral little iſlands raiſed out of the ſea by a ſubmarine volcano. The eruption of one of theſe in the year 1708, with all the circumſtances that attended it, we have a very good account of in the † Philoſophical Tranſactions. It was raiſed in a place where the ſea had been formerly 100 fathoms deep, and was attended with earthquakes before it ſhewed itſelf above water, as well as after. It is reported, that the iſland of Santerini itſelf was originally raiſed out of the ſea in the ſame manner; but, be that as it will, we have certain accounts of new iſlands raiſed there, or additions made to the old ones, from time to time, for above 1900 years backwards, and there have always been earthquakes at the time of theſe eruptions.

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* See Hiſt. and Philoſ. of Earthquakes, under the titles Azores, Iſlands raiſed, &c.

† See N° 314, 317, and 332. or vol. v. part ii. p. 196. Jones's Abr.

31. Another example of the ſame kind happened at * Manila, one of the Philippine iſlands, in the year 1750. This alſo was attended with violent earthquakes, to which that iſland, as well as the reſt of the Philippines, is very much ſubject.

32. We may add to theſe, the many inſtances of vaſt quantities of † pumice ſtones, which have been ſometimes found floating upon the ſea, at ſo great a diſtance from the ſhore, as well as from any known volcano, that there can be little doubt of their being thrown up by fires ſubſiſting under the bottom of the ocean.

33. From theſe inſtances, we may, with great probability, conclude, that the fires of volcanos produce earthquakes: I do not, however, ſuppoſe, that the earthquakes, which are frequently felt in the neighbourhood of volcanos, are owing to the fires of thoſe volcanos themſelves; for volcanos, giving paſſage to the vapours that are there formed, ſhould rather prevent them, as in the inſtance at St. Chriſtopher's, before-mentioned.

34. We alſo meet with frequent inſtances confirming the ſame thing amongſt the Andes. Antonio d'Ulloa (ſpeaking of what happens amongſt theſe mountains) ſays, "Experience ſhews us, that, upon "the freſh breaking out of any volcano, it occaſions "ſo violent a ſhock to the earth, that all the villages, "which are near it, are overthrown and deſtroyed,

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* See Philoſ. Tranſ. vol. xlix. p. 459.

† See Philoſ. Tranſ. N° 372. or vol. vi. part ii. p. 204. and N° 402. or vol. vii. part ii. p. 43. Eames's Abr.

" as it happened in the caſe of the mountain * Car-
" guayraſo. This ſhock, which we may, without
" the leaſt impropriety, call an earthquake, is ſel-
" dom found to accompany the eruptions, after an
" opening is once made; or, if ſome ſmall trembling
" is perceived, it is very inconſiderable; ſo that,
" after the volcano has once found a vent, the ſhocks
" ceaſe, notwithſtanding the matter of it continues
" to be on fire." The greater earthquakes, there-
fore, ſeem rather to be occaſioned by other fires, that lie deeper in the ſame tract of country; and the eruptions of volcanos, which happen at the ſame time with earthquakes, may, with more probability, be aſcribed to thoſe earthquakes, than the earthquakes to the eruptions, whenever, at leaſt, the earthquakes are of any conſiderable extent. If this don't appear ſufficiently manifeſt at preſent, it will, perhaps, be better underſtood, by applying to the preſent purpoſe, what will be ſaid hereafter concerning local earthquakes.

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* It does not appear altogether certain, from the expreſſion made uſe of in the French tranſlation (from whence I have taken this), that Carguayraſo might not have been a volcano in former times, which is aſſerted to have been the caſe by Monſ. Condamine. It is poſſible alſo, that the ſame may be true of thoſe four mentioned in the next article; and, indeed, it is difficult to know it to be otherwiſe, in any inſtance, among the Andes, where the volcanos are generally found at inacceſſible heights. But allowing, that all theſe were only old volcanos, which broke out afreſh, yet they will ſerve at leaſt to ſwell the number of them in the ſame neighbourhood, as well as to ſhew us, that there may, very probably, be many more, which lie hid: for theſe ſhewed no marks of their exiſtence, till, by their eruption, they melted a vaſt quantity of ſnow, with which they were before covered, and which, being reduced to water, did great damage, by overflowing the country round about.

SECT.

SECT. III.

35. It may be aſked, perhaps, why we ſhould ſuppoſe, that ſeveral ſubterraneous fires exiſt in the neighbourhood of volcanos? In evidence of this, we have frequent inſtances of new volcanos breaking out in the neighbourhood of old ones: Carguayraſo, juſt mentioned, may ſupply us with one example to this purpoſe; and, in the night of the 28th of October 1746, in which Lima and Callao were deſtroyed, no leſs than four * new ones burſt forth in the adjacent mountains.

36. To the ſame purpoſe, we may allege the inſtances of many volcanos lying together in the ſame tract of country: as for example, the many places, "not ſo few as forty," amongſt the Azores, which either do now or have formerly ſent forth ſmoke and flames; the many volcanos alſo amongſt the Andes, already mentioned: thus Ætna, Strombolo, and Veſuvius, I may add Solfatara too, are all in the ſame neighbourhood: and Monſ. Condamine ſays, he has traced † lavas, exactly like thoſe of Veſuvius, all the way from Florence to Naples. In ‡ Iceland alſo, we have, beſides Hæcla, not only ſeveral other volcanos, but alſo a great number of places, that ſend up ſul-

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* See d'Ulloa's Voyage to Peru, part ii. book i. chap. 7.

† See Phil. Tranſ. vol. xlix. p. 624. All theſe lavas, as well as the volcanos juſt mentioned, lie in a continued line. The ſame thing holds good in the volcanos of the Andes alſo. This is a fact I muſt deſire the reader to attend to, as it ſerves to confirm a very material doctrine, which I ſhall have occaſion to mention hereafter. See art. 44, 45, and 46.

‡ See Horrebow's Natural Hiſtory of Iceland.

phureous vapours. But the examples of this kind are ſo frequent, that there are few inſtances to be produced of ſingle volcanos, without evident marks, either that there have been others formerly in their neighbourhood, or that there are, at preſent, ſubterraneous fires near them.

37. This frequency of ſubterraneous fires, in the neighbourhood of volcanos, will appear ſtill more probable, if we conſider the internal ſtructure of the earth; and, as it will be neceſſary alſo, in order to underſtand what follows, to know a little more of this matter, than what falls under common obſervation, I ſhall endeavour to give the reader ſome account of it.

38. The earth then (as far as one can judge from the appearances), is not compoſed of heaps of matter caſually thrown together, but of regular and uniform ſtrata. Theſe ſtrata, though they frequently do not exceed a few feet, or perhaps a few inches, in thickneſs, yet often extend in length and breadth for many miles, and this without varying their thickneſs conſiderably. The ſame ſtratum alſo preſerves a uniform character throughout, though the ſtrata immediately next to each other are very often totally different. Thus, for inſtance, we ſhall have, perhaps, a ſtratum of potters clay; above that, a ſtratum of coal; then another ſtratum of ſome other kind of clay; next, a ſharp grit ſand ſtone; then clay again; next, perhaps, ſand ſtone again; and coal again above that; and it frequently happens, that none of theſe exceed a few yards in thickneſs. There are, however, many inſtances, in which the ſame kind of matter is extended to the depth of ſome hundreds

hundreds of yards; but in all theſe, a very few only excepted, the whole of each is not one continued maſs, but is again ſubdivided into a great number of thin laminæ, that ſeldom are more than one, two, or three feet thick, and frequently not ſo much.

39. Beſide the horizontal diviſion of the earth into ſtrata, theſe ſtrata are again divided and ſhattered by many perpendicular fiſſures, which are in ſome places few and narrow, but oftentimes many, and of conſiderable width. There are alſo many inſtances, where a particular ſtratum ſhall have almoſt no fiſſures at all, though the ſtrata both above and below it are conſiderably broken: this happens frequently in clay, probably on account of the ſoftneſs of it, which may have made it yield to the preſſure of the ſuperincumbent matter, and fill up thoſe fiſſures which it originally had; for we ſometimes meet with inſtances in mines, where the correſpondent fiſſures in an upper and lower ſtratum are interrupted in an intermediate ſtratum compoſed of clay, or ſome ſuch ſoft matter.

40. Though theſe fiſſures do ſometimes correſpond to one another in the upper and lower ſtrata, yet this is not generally the caſe, at leaſt not to any great diſtance: thoſe clefts, however, in which the larger veins of the ores of metals are found, are an exception to this obſervation; for they ſometimes paſs through many ſtrata, and thoſe of different kinds, to unknown depths.

41. From this conſtitution of the earth, *viz.* the want of correſpondence in the fiſſures of the upper and lower ſtrata, as well as on account of thoſe ſtrata which are little or not at all ſhattered, it will come to paſs,

paſs, that the earth cannot eaſily be ſeparated in a direction * perpendicular to the horizon, if we take any conſiderable portion of it together; but in the horizontal direction, as there is little or no adheſion between one ſtratum and another, it may be ſeparated without difficulty.

42. Thoſe fiſſures which are at ſome depth below the ſurface of the earth, are generally found full of water; but all thoſe that are below the level of the ſea, muſt always be ſo, either from the oozing of the ſea, or rather of the land waters between the ſtrata.

43. The ſtrata of the earth are frequently very much bent, being raiſed in ſome places, and depreſſed in others, and this ſometimes with a very quick aſcent or deſcent; but as theſe aſcents and deſcents, in a great meaſure, compenſate one another, if we take a large extent of country together, we may look upon the whole ſet of ſtrata, as lying nearly horizontally. What is very remarkable, however, in their ſituation, is, that from moſt, if not all, large tracts of high and mountainous countries, the ſtrata lie in a ſituation more inclined to the horizon, than the country itſelf, the † mountainous countries being generally,

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* What I ſaid before of thoſe deep clefts, in which metals are found, will not affect this concluſion; for they are conſiderably different from either perpendicular or plane ſections of earth; they are frequently interrupted by ſtrata of clay, or other ſoft matter; and they are, in moſt parts, either filled up with rubbiſh, or with ores and ſpars, that adhere as firmly to the rocks on both ſides, as if they compoſed one continued ſtratum with them.

† It ſeems very probable, from many appearances, not only that the mountainous countries are formed out of the lower ſtrata of the earth, but that ſometimes the higheſt hills in them are formed out

Fig. 1.



Fig. 3.

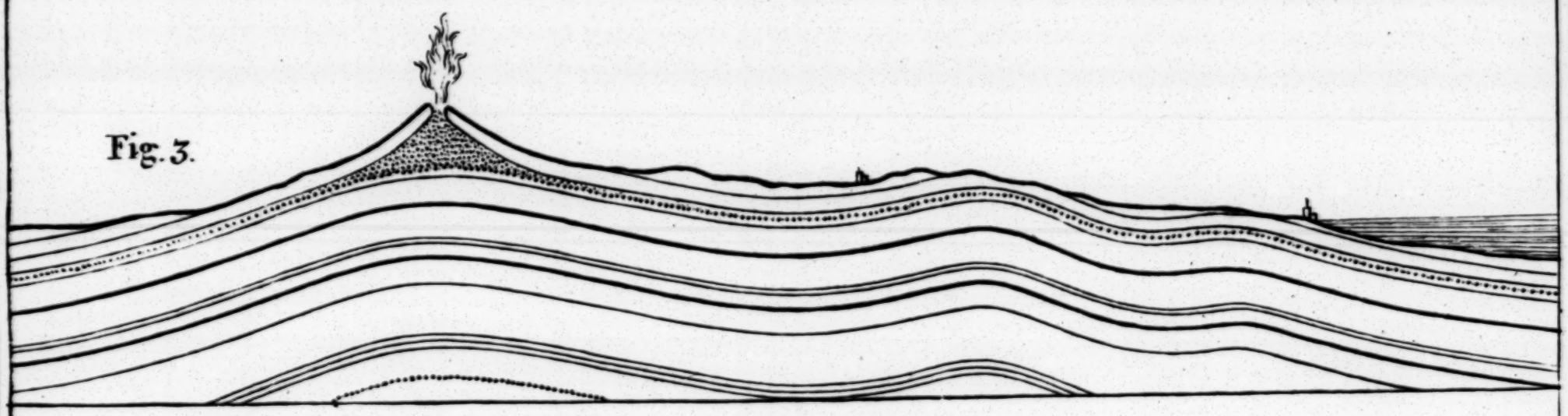


Fig. 4.

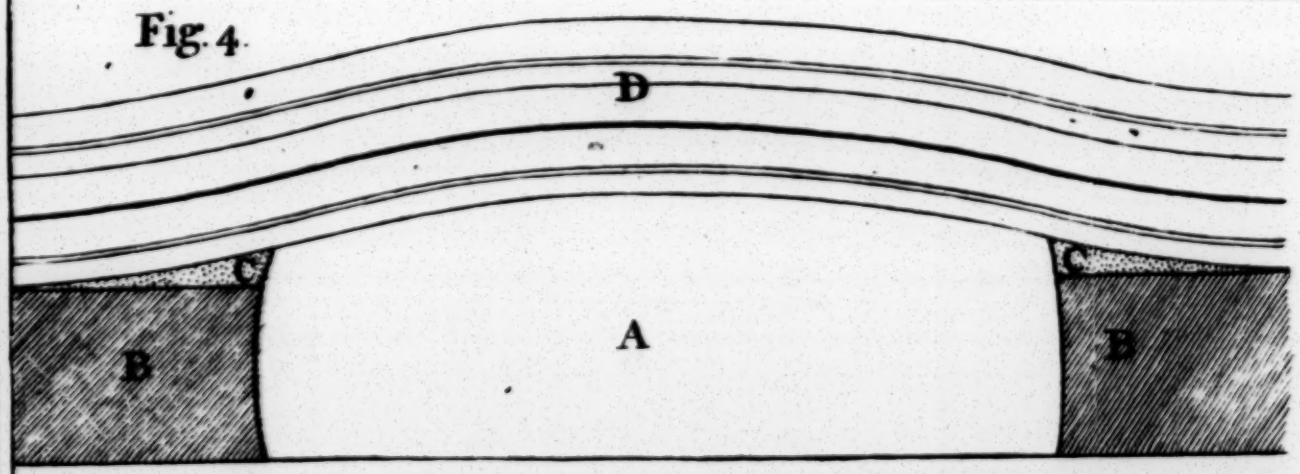
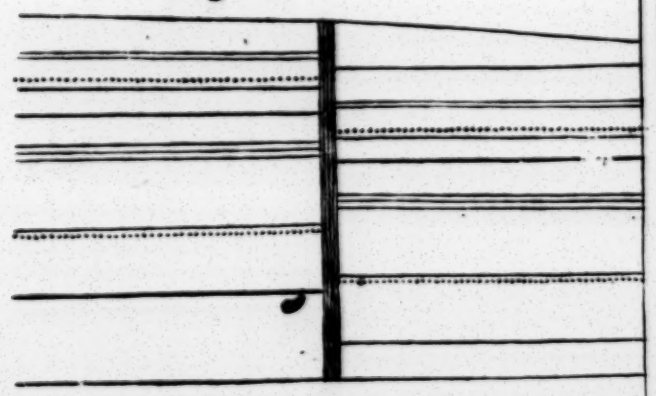


Fig. 2.



J. Mynde Sc.

generally, if not always, formed out of the lower ſtrata of earth. This ſituation of the ſtrata may be not unaptly repreſented in the following manner. Let a number of leaves of paper, of ſeveral different ſorts or colours, be paſted upon one another; then bending them up together into a ridge in the middle, conceive them to be reduced again to a level ſurface, by a plane ſo paſſing through them, as to cut off all the part that had been raiſed; let the middle now be again raiſed a little, and this will be a good general repreſentation of moſt, if not of all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world *.

44. From this formation of the earth, it will follow, that we ought to meet with the ſame kinds of earths, ſtones, and minerals, appearing at the ſurface, in long narrow ſlips, and lying parallel to the greateſt riſe of any long ridges of mountains; and ſo, in fact, we find them. The Andes in South America, as it has been ſaid before, have a chain of volcanos, that extend in length above 5000 miles: theſe volcanos, in all probability, are all derived from the † ſame

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out of ſtrata ſtill lower than the reſt, which, perhaps, may always be the caſe, where they have volcanos in them. [See a repreſentation of this in Tab. XIII. Fig. 3.] In other inſtances, however, it often happens, that the hills, to which theſe high lands ſerve as a baſe, are not only formed out of the ſtrata next above them, but they ſtand, as it were, in a diſh, as if they had depreſſed the ground, on which they reſt, by their weight.

* Fig. 1. repreſents a ſection of a ſett of ſtrata, lying in the ſituation juſt deſcribed: the ſection is ſuppoſed to be made at right angles to the length of the ridge, and perpendicular to the horizon.

† See the notes to art. 36 and 53. See alſo Fig. 3.

ſtratum.

ſtratum. Parallel to the Andes, is the Sierra, another long ridge of mountains, that run between the Andes and the ſea; and "theſe two ridges of mountains run "within ſight of one another, and almoſt equally, "for above a thouſand leagues together *," being each, at a medium, about twenty leagues wide. The gold and ſilver mines wrought by the Spaniards, are found in a tract of country parallel to the direction of theſe, and extending through a great part of the length of them.

45. The ſame thing is found to obtain in North America alſo. The great lakes, which give riſe to the river St. Laurence, are kept up by a long ridge of mountains, that run nearly parallel to the eaſtern coaſt. In deſcending from theſe towards the ſea, the ſame ſets † of ſtrata, and in the ſame order, are generally met with throughout the greateſt part of their length.

46. In Great Britain, we have another inſtance to the ſame purpoſe, where the direction of the ridge varies about a point from due north and ſouth, lying nearly from ‡ N. by E. to S. by W. There are many more inſtances of this to be met with in the world, if we may judge from circumſtances, which make it highly probable, that it obtains in a great number of places, and in ſeveral they ſeem to put it almoſt out of doubt.

47. The reader is not to ſuppoſe, however, that, in any inſtances, the higheſt riſe of the ridge, and

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* See Acoſta's Natural Hiſtory of the Indies.

† See Lewis Evans's Map and Account of North America.

‡ Of this I could give many undoubted proofs, if it would not too far exceed the limits of my preſent deſign, and which, for that reaſon, I am obliged to omit.

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the inclination of the ſtrata from thence to the countries on each ſide, is perfectly uniform; for they have frequently very conſiderable inequalities, and theſe inequalities are ſometimes ſo great, that the ſtrata are bent for ſome ſmall diſtance, even the contrary way from the general inclination of them. This often makes it difficult to trace the appearance I have been relating, which, without a general knowlege of the foſſil bodies of a large tract of country, it is hardly poſſible to do.

48. At conſiderable diſtances from large ridges of mountains, the ſtrata, for the moſt part, aſſume a ſituation nearly level; and as the mountainous countries are generally formed out of the lower ſtrata, ſo the more level countries are generally formed out of the upper ſtrata of the earth.

49. Hence it comes to paſs, that, in countries of this kind, the ſame ſtrata are found to extend themſelves a great way, as well in breadth as in length: we have an inſtance of this in the chalky and flinty countries of England and France, which (excepting the interruption of the Channel, and the clays, ſands, *&c.* of a few counties) compoſe a tract of about three hundred miles each way.

50. Beſides the raiſing of the ſtrata in a ridge, there is another very remarkable appearance in the ſtructure of the earth, though a very common one; and this is what is uſually called by miners, the trapping down of the ſtrata; that is, the whole ſet of ſtrata on one ſide a cleft are ſunk down below the level of the correſponding ſtrata on the other ſide. If, in ſome caſes, this difference in the level of the ſtrata, on the different ſides of the cleft, ſhould be

very conſiderable, it may have a great effect in producing ſome of the ſingularities of particular earthquakes *.

PART II.

51. IN the former part of this eſſay, I have recounted ſome of the principal appearances of earthquakes, as well as thoſe particulars in the ſtructure of the earth, upon which I ſuppoſe theſe appearances to depend. From what has been already ſaid, I think it is ſufficiently manifeſt, that, in ſome inſtances at leaſt, earthquakes are actually produced by ſubterraneous fires; it now, therefore, remains to be ſhewn, how all the appearances above-recited, as well as many other minuter circumſtances attending earthquakes, may be accounted for from the ſame cauſe.

SECT. I.

52. The returns of earthquakes in the ſame places, either at ſmall or large intervals of time, are very conſiſtent with the cauſe aſſigned: ſubterraneous fires, from their analogy to volcanos, might reaſonably be ſuppoſed to ſubſiſt for many ages, though we had not thoſe inſtances † already mentioned, which put the

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* Fig. 2. repreſents a ſection of the ſtrata ſlipping down after the manner juſt deſcribed. The ſection is ſuppoſed to be made perpendicularly to the horizon, and at right angles to the direction of the cleft: an inſtance of this kind, amongſt the coal miners of Mendip in Somerſetſhire, is mentioned in the Philoſ. Tranſ. See the account of it, together with a drawing, in N° 360. or Jones's Abr. vol. iv. part ii. p. 260.

† See art. 28 to 32 incluſive.

matter

matter out of doubt. And, as it frequently happens, that volcanos rage for a time, and then are quiet again for a number of years; ſo we ſee earthquakes alſo frequently repeated for ſome ſmall time, and then ceaſing again for a long term, excepting, perhaps, now and then ſome ſlight ſhock. And this analogy between earthquakes, and the effects of volcanos, is ſo great, that I think it cannot but appear ſtriking to any one, who will read the accounts of both, and compare them together. The raging of volcanos is not one continued and uniform effect; but an effect, that is repeated at unequal intervals, and with unequal degrees of force: thus, for inſtance, we ſhall have, perhaps, two or three blaſts diſcharged from a volcano, ſucceeding one another at the interval of a few ſeconds only: ſometimes the intervals are of a quarter of an hour, an hour, a day, or perhaps ſeveral days. And as theſe intervals are very unequal, ſo is the violence of the blaſts alſo: ſometimes ſtones, *&c.* are thrown, by theſe blaſts, to the diſtance of ſome miles; at other times, perhaps, not to the diſtance of a hundred yards. The ſame difference is obſerved in the intervals and violence of the ſhocks of earthquakes, which are repeated at ſmall intervals for ſome time.

Sect. II.

53. The great frequency of earthquakes in the neighbourhood of burning mountains, is a ſtrong argument of their proceeding from a cauſe of the ſame kind: and the analogy of ſeveral volcanos lying together in the ſame tract of country, as well as new ones breaking out in the neighbourhood of old ones,

tends greatly to confirm this opinion; but what makes it ſtill the more probable, is that peculiarity in the ſtructure of the earth, already mentioned. I obſerved before, that the ſame ſtrata are generally very extenſive, and that they commonly lie more inclining from the mountainous countries, than the countries themſelves: theſe circumſtances make it very probable, that thoſe * ſtrata of combuſtible materials, which break

* It has been imagined by ſome authors, that volcanos are produced by the pyrites of veins, and that they do not owe their origin to the matter of ſtrata. In order to prove this, it is alleged, that volcanos are generally found on the tops of mountains, and that thoſe are the places in which veins of pyrites are generally lodged. This argument being taken from obſervations that have their foundation in nature, ought not to go unanſwered. In the firſt place, then, the pyrites of veins, or fiſſures, are not found in ſufficient quantities, or extending to a ſufficient breadth, to be ſuppoſed capable of producing the fires of volcanos: it very rarely happens, that we meet with a vein or fiſſure five or ſix yards wide; and when we meet with ſuch an one, yet, perhaps, not a twentieth part of it at moſt ſhall be filled with pyrites; but the fires of volcanos, inſtead of being long and narrow, as if the matter that ſupplied them was depoſited in veins, are generally round, and of far greater breadth than veins can be ſuppoſed to be. Monſ. Bouguer ſays, that the mouth of the volcano Cotopaxi is, at this time, five or ſix hundred fathoms wide; [ſee Hiſt. and Phil. of Earthquakes, p. 195.] and the burning iſland that was raiſed out of the ſea near Tercera, as before-mentioned, was almoſt three leagues in diameter, and nearly round. [See art. 29.]

Beſides this, it is very difficult to conceive how any matters lodged in veins can ever take fire; for, excepting where the veins are extremely narrow, they are almoſt always drowned in a very great quantity of water, which has free acceſs to every part of them: neither are the pyrites of veins, by any means, ſo apt to take fire of themſelves, as thoſe of ſtrata; and if, indeed, there are any of them that will do ſo, yet they are but few in compariſon of thoſe which will not: all thoſe, which, beſide iron and ſulphur, contain copper, or arſenic, even in a very ſmall proportion, are not

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break out in volcanos on the tops of the hills, are to be found at a conſiderable depth under ground in the level and low countries near them. If this ſhould be the caſe,

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at all ſubject to inflame of themſelves. On the other hand, moſt of the pyrites of ſtrata, if not all of them, have this property more or leſs. There are alſo two ſorts of ſtrata, in which pyrites are lodged in the greateſt abundance, that have the ſame property, and that frequently in as great a degree as themſelves: theſe are coals and aluminous earths, or ſhale. There are ſome kinds of both theſe, that, upon being expoſed to the external air for a few months, will take fire of themſelves, and burn. Theſe two ſorts of ſtrata are alſo near akin to each other; they are generally found to accompany each other; they are both of them generally intermixed with, or accompanied by ſtrata of iron ore; and they both of them, for the moſt part, either contain, or are lodged amongſt, the remains of vegetable bodies; and theſe remains of vegetable bodies, in the aluminous earths, are frequently either wholly, or in part, converted into pyrites, or coal, or both. Numberleſs, inſtances of this are to be met with in the aluminous ſhale of Whitby and other places.

It is very probable, that to ſome ſtratum of this kind the fires of volcanos are owing; and this ſeems to be confirmed by the ſimilarity of the materials, which are thrown up or ſublimated by the fires of volcanos, to the matter of the aluminous earths. Solfatara produces ſulphur, alum, and ſal ammoniac. The two former of theſe are very eaſily to be obtained from the aluminous earths, and, I ſuppoſe, the latter alſo; at leaſt it is procurable from the ſoot of common foſſil coals, and probably, therefore, from the ſoot of that coaly matter which is intermixed with ſuch earths.

The aluminous earths, moreover, not only have ſeveral ſtrata of iron ore lying in them, but they alſo contain a conſiderable proportion of iron in their compoſition. In correſpondence to this, we find the lavas of volcanos, and other matters thrown out from thence, frequently containing a great deal of iron, the ſmall duſt of them readily adhering to the magnet.

As to the pyrites of veins, I much doubt whether they ever contain alum, or ſal ammoniac; at leaſt they are very rarely found to contain either the one or the other.

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and if the ſame * ſtrata ſhould be on fire in any places under ſuch countries, as well as on the tops of the hills, all vapours, of whatſoever kind, raiſed from theſe fires, muſt be pent up, unleſs ſo far as they can open themſelves a paſſage between the ſtrata; whereas the vapours raiſed from volcanos find a vent, and are diſcharged in blaſts from the mouths of them. Now, if, when they find ſuch a vent, they are yet capable of ſhaking the country to the diſtance of ten or twenty miles round, what may we not expect from them, when they are confined? We may form ſome idea of the force and quantity of theſe vapours from their effects: it is no uncommon thing to ſee them throw up, at once, ſuch clouds of ſand, aſhes, and pumice ſtones, as are capable of darkening the whole air, and covering the neighbouring country with a ſhower of duſt, *&c.* to ſome miles diſtance: great ſtones alſo, of ſome tons weight, are often thrown to the diſtance of two or three miles by theſe exploſions: and Monſ. Bouguer tells us, that he met with ſtones

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* It may be aſked, perhaps, why a ſtratum liable to take fire in ſome places, ſhould not take fire throughout the whole extent of it? In anſwer to this, it may be ſaid, that the ſame ſtratum may differ a little in the richneſs of its combuſtible principles in different places; or, perhaps, the frequency of the fiſſures, either in the combuſtible ſtratum itſelf, or the ſtratum next to it, may let in ſo much water, as to prevent its taking fire, excepting in a few places; but, if this once happens, the fire will not eaſily be put out again, but it will ſpread itſelf, notwithſtanding the fiſſures that lie in its way, though they are filled with water; for the matter on fire will be, in ſome degree at leaſt, in a fluid ſtate; and, for this reaſon, it muſt neceſſarily expel the water from the fiſſures, both on account of the extenſion of its own dimenſions by the heat, and of the weight of the ſuperincumbent earth, which, preſſing it, will make it ſpread laterally.

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in South America, of eight or nine feet diameter, that had been thrown from the volcano Cotopaxi, by one of theſe blaſts, to the diſtance of more than * three leagues.

54. If we ſuppoſe that theſe vapours, when pent up, are the cauſe of earthquakes, we muſt naturally expect, from what has been juſt ſaid, that the moſt extenſive earthquakes ſhould take their riſe from the level and low countries; but more eſpecially from the ſea, which is nothing elſe than waters covering ſuch countries. Accordingly we find, that the great earthquake of the 1ſt November 1755, which was felt at places near three thouſand miles diſtant from each other, took its riſe from under the ſea; this is manifeſt, from that wave which accompanied it, as ſhall be ſhewn hereafter. The ſame thing is to be underſtood of the earthquake that deſtroyed Lima in the year 1746, which, it has been ſaid, was felt as far as Jamaica; and, as it was more violent than the Liſbon earthquake, ſo, if this be true, it muſt, in all probability, have been more extenſive alſo. There have been many other very extenſive earthquakes in South America: Acoſta ſays, that they have been often known to extend themſelves one, two, or three hundred, and ſome even five hundred leagues, along the coaſt. Theſe have been generally, if not always, attended with waves from the ſea; but any minuter

* See Hiſt. and Philoſ. of Earthq. p. 195. Don Antonio d'Ulloa, an author of great veracity, ſpeaking of the ſame thing, ſays, that "the whole plain [near Latacunga] is full of large pieces of rocks, "ſome of them thrown from the volcano Cotopaxi, by one of its "eruptions, to the diſtance of five leagues." See his Voyage to Peru, part i. book vi. chap. 1.

circum-

circumſtances accompanying them are not related. Indeed it is hardly to be expected that they ſhould be obſerved, much leſs that they ſhould be related, when they happened in a country ſo thinly inhabited, and where one may reaſonably ſuppoſe, that, in general, only the groſſer and more violent effects would be taken notice of.

SECT. III.

55. I have ſaid before, that I imagined earthquakes were cauſed by vapours raiſed from waters ſuddenly let out upon ſubterraneous fires. It is not eaſy to find any other cauſe capable of producing ſuch ſudden and violent effects, or of raiſing ſuch an amazing quantity of vapour in ſo ſmall a time. That the blaſts, diſcharged from volcanos, are always produced from this cauſe, is highly probable; that they are often ſo, cannot admit of the leaſt doubt. There can be no doubt, that conſiderable quantities of water muſt be often let out upon the fires of theſe volcanos, and whenever this happens, it will be immediately raiſed by the heat of them into a vapour, whoſe elaſtic force is capable of producing the moſt violent effects *.

56. Both

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* There are many effects produced by the vapour of water, when intenſely heated, which make it probable, that the force of gunpowder is not near equal to it. The effects of an exceeding ſmall quantity of water, upon which melted metals are accidentally poured, are ſuch, as, I think, could in no wiſe be expected from the like quantity of gunpowder. Founders, if they are not careful, often experience theſe effects to their coſt. An accident of this kind happened about forty years ſince, at the caſting of two braſs cannon at Windmill-hill, Moorfields. "The "heat

56. Both the tremulous and wave-like motion obſerved in earthquakes, may be very well accounted for

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" heat of the metal of the firſt gun drove ſo much damp into the " mould of the ſecond, which was near it, that as ſoon as the " metal was let into it, it blew up with the greateſt violence, tear- " ing up the ground ſome feet deep, breaking down the furnace, " untiling the houſe, killing many ſpectators on the ſpot, with the " ſtreams of melted metal, and ſcalding many others in a moſt mi- " ſerable manner." [See the note at the end of proceſs 44th of the Engliſh tranſlation of Cramer's Art of aſſaying Metals.]

Other inſtances of the violence of vapours raiſed from water, are frequently to be met with: one of Papin's digeſters being placed between the bars of a grate, where there was a fire, was, after ſome time, burſt by the violence of the ſteam, the fire was all blown out of the grate, and a piece of the digeſter was driven againſt the leaf of a ſtrong oak table, which it broke to pieces. [See Philoſ. Tranſ. N° 454. or Martyn's Abr. vol. viii. p. 465.] The marquis of Worceſter alſo, in his Century of Inventions, tells us, that he burſt a cannon by the ſame means.

It has been ſometimes imagined, that the vapours, which occaſion earthquakes, were of the ſame kind with thoſe fulminating damps, of which we often meet with inſtances in coal mines. Now, there are ſeveral things which make it very probable, that this is not the caſe: it is true, the force of ſuch vapours is very great; we have had inſtances, where large beams of timber have been thrown to the diſtance of an hundred yards by them: [ſee Philoſ. Tranſ. N° 136. or vol. ii. p. 381. Lowthorp's Abr.] but what is this to the force of that vapour, which could throw ſtones of twenty or thirty ton weight to the diſtance of three leagues? Nor, indeed, is it at all probable, that any vapour, already in the form of a vapour, can, by ſuddenly taking fire, increaſe its dimenſions ſo much, as to produce that immenſe quantity of motion, which we obſerve in ſome earthquakes: but this is rather to be expected from ſome ſolid body, ſuch as water, which is capable of being converted, and that almoſt inſtantly, into one of the lighteſt, and perhaps one of the moſt elaſtic, vapours in the world. Air, when heated to the greateſt degree that it is capable of receiving from the hotteſt fires we can make, acquires a degree of elaſticity about five times as great as that of common air: the vapour of

for from ſuch a vapour. In order to trace a little more particularly the manner in which theſe two motions

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gunpowder, whilſt it is inflamed, has alſo about five times the elaſtic force which it has when cold. [See Robins's excellent tract on Gunnery.] Now, if we ſuppoſe a fulminating damp, of any kind, to increaſe its elaſticity, when inflamed in the ſame proportion, this will be abundantly ſufficient to make it produce any effects, which we have ever ſeen produced by any of the damps of mines, &c. And, indeed, whoever carefully examines the effects, either of the damps of mines, or of thoſe fulminating damps, that are raiſed from ſome metals, when in fuſion, or when they are diſſolving in acids, will rather be inclined to think, that the force of inflamed vapours is ſo far from exceeding the proportion of five to one, that it falls conſiderably ſhort of it.

But though we ſhould ſuppoſe that this proportion holds good, where ſhall we find a place capable of containing a ſufficient quantity of ſuch a vapour, to produce the great effects of earthquakes? It will be ſaid, perhaps, in ſubterraneous caverns. To this we may anſwer, that he, who is but moderately acquainted with the ſtructure of the earth, and the materials of which it is compoſed, will be little inclined to allow of any great or extenſive caverns in it. But, though this ſhould be admitted, how can it come to paſs that theſe caverns ſhould not be filled with water? If it is alleged, that the water is expelled, as the vapour is formed, why ſhould not the vapour, as it is ſuppoſed to be the lighter, be expelled, rather than the water, by the ſame paſſages by which the water is to be expelled? But let us ſuppoſe this difficulty alſo to be got over, and the water to be removed, and we ſhall then have a gage for the denſity of the vapour; for it muſt be juſt ſufficient to make it capable of ſuſtaining a column of water, whoſe height is equal to that of the ſurface of the ſea above the bottom of the cavern, in which the vapour is ſuppoſed to be contained. Now, ſince the mean weight of earth, ſtones, &c. is not leſs than two and a half times the weight of water, this vapour muſt be increaſed to two and half times its original elaſticity, before it can, in any wiſe, raiſe the earth above it; and if we ſuppoſe it to be increaſed to five times its original elaſticity, it will then be no more than twice able to do ſo; in which caſe, ſo much vapour only can be diſcharged from the cavern, to produce an earthquake, as is equal to the content

motions will be brought about, let us ſuppoſe the roof over ſome ſubterraneous fire to fall in. If this ſhould be the caſe, the earth, ſtones, *&c.* of which it was compoſed, would immediately ſink in the melted matter of the fire below: hence all the water contained in the fiſſures and cavities of the part falling in, would come in contact with the fire, and be almoſt inſtantly raiſed into vapour. From the firſt effort of this vapour, a cavity would be formed (between the melted matter and ſuperincumbent earth) filled with vapour only, before any motion would be perceived at the ſurface of the earth: this muſt neceſſarily happen, on account of the * compreſſibility of

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content of the cavern: and what muſt the ſize of that cavern be, which could contain vapour enough to produce the earthquake of the 1ſt of November 1755, in which an extent of earth of near three thouſand miles diameter was conſiderably moved? or how can we ſuppoſe, that the roof of ſuch a cavern, when ſo violently ſhaken, ſhould avoid falling in? eſpecially, as it is hardly to be ſuppoſed, that any inflamed vapour whatſoever ſhould be able to move the earth over theſe caverns, if they lay at any great depth, ſince the weight of leſs than three miles depth of earth is capable of retaining the inflamed vapour of gunpowder within the original dimenſions of the gunpowder itſelf; and common air, compreſſed by the ſame weight (ſuppoſing the known law of its compreſſion to hold ſo far), would be of greater denſity than water.

We may aſk ſtill farther, whence ſuch vaſt quantities of vapour ſhould be formed, or what ſources they muſt be, which would not be exhauſted (if they were not again repleniſhed) by a very few repetitions of ſuch immenſe diſcharges.

* The compreſſibility and elaſticity of the earth, are qualities which don't ſhow themſelves in any great degree in common inſtances, and therefore are not commonly attended to. On this account it is, that few people are aware of the great extent of them, or the effects that may ariſe from them, where exceeding large quantities of matter are concerned, and where the compreſ-

of all kinds of earth, ſtones, &c. but as the compreſſion of the materials immediately over the cavity, would

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ſive force is immenſely great. The compreſſibility and elaſticity of the earth may be collected, in ſome meaſure, from the vibration of the walls of houſes, occaſioned by the paſſing of carriages in the ſtreets next to them. Another inſtance to the ſame purpoſe, may be taken from the vibrations of ſteeples, occaſioned by the ringing of bells, or by guſts of wind: not only ſpires are moved very conſiderably by this means, but even ſtrong towers will, ſometimes, be made to vibrate ſeveral inches, without any disjointing of the mortar, or rubbing of the ſtones againſt one another. Now, it is manifeſt, that this could not happen, without a conſiderable degree of compreſſibility and elaſticity in the materials, of which they are compoſed: and if ſuch ſmall things as the weight of ſteeples, and the motion of bells in them, or a guſt of wind, are capable of producing ſuch effects, what may we not expect from the weight of great depths of earth? There are ſome circumſtances, which ſeem to make it not altogether improbable, that the form and internal ſtructure of the earth depend, in a great meaſure, upon the compreſſibility and elaſticity of it. There are ſeveral things that ſeem to argue a conſiderably greater denſity in the internal, than the external part of the earth; and why may not this greater denſity be owing to the compreſſion of the internal parts ariſing from the weight of the ſuperincumbent matter, ſince it is probable, that the matter, of which the earth is compoſed, is pretty much of the ſame kind throughout? There is a ſtill ſtronger argument for the earth's owing its form, in ſome meaſure, to the ſame cauſe; for it is found to be higher [ſee the French accounts of the meaſures of a degree of the meridian in France, Sweden, and America] at the æquator, than at the poles, in a greater proportion than it would be on account of the centrifugal force, if it was of uniform denſity; but, if we ſuppoſe the earth to be of leſs denſity in an æquatorial diameter than in the axis, the whole will then be eaſily accounted for, from the riſing of the earth a little by its elaſticity, the weight being in part taken off by the diurnal rotation: and that the earth is really a little denſer in the axis, than in the æquatorial diameter, ſeems highly probable, from the experiments of pendulums compared with aſtronomical obſervations; for the forms of the earth derived from theſe, cannot be reconciled

would be more than ſufficient to make them bear the weight of the ſuperincumbent matter, this compreſſion muſt be propagated on account of the elaſticity of the earth, in the ſame manner as a pulſe is propagated through the air; and again the materials immediately over the cavity, reſtoring themſelves beyond their natural bounds, a dilatation will ſucceed to the compreſſion; and theſe two following each other alternately, for ſome time, a vibratory motion will be produced at the ſurface of the earth. If theſe alternate dilatations and compreſſions ſhould ſucceed one another at very ſmall intervals, they would excite a like motion in the air, and thereby occaſion a conſiderable noiſe. The noiſe that is uſually obſerved to precede or accompany earthquakes, is probably owing partly to this cauſe, and partly to the grating of the parts of the earth together, occaſioned by that wave-like motion before-mentioned.

57. After the water, that firſt came in contact with the fire, has formed a cavity, all the reſt of the water contained in the fiſſures, immediately communicating with the hollow left by the part that fell in,

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reconciled with each other, but upon this ſuppoſition. [See Maclaurin's Fluxions, art. 681, &c.] It appears, from ſome late and accurate obſervations, that the æquatorial parts of the planet Jupiter alſo, as well as thoſe of the earth, are a little higher than they would be, if their riſe was owing to the centrifugal force, and he was of uniform denſity; but if we ſuppoſe him to be of leſs denſity in the æquatorial, than the polar regions, then the form may be ſuch as he would aſſume from the reſpective gravitation of the ſeveral parts; and any fluid like our ocean, would not overflow the polar parts, (which, upon any other ſuppoſition, it muſt neceſſarily do) but would follow his general form, as our ocean does that of the earth.

muſt run out upon the fire, the ſteam taking its place. From hence may be generated a vaſt quantity of vapour, the effects of which ſhall be conſidered preſently. This ſteam will continue to be generated, ſuppoſing the fire to be ſufficiently great, till the fiſſures before-mentioned are evacuated, or till the water begins to flow very ſlowly; when the ſteam already formed will be removed by the elaſticity of the earth, which will again ſubſide, and, preſſing upon the ſurface of the melted matter, will force it up a little way into all the clefts, by which the water might continue to flow out. By this means, all communication between the fire and the water will be prevented, excepting at theſe clefts, where the water, dripping ſlowly upon the melted matter, will gradually form a cruſt upon it, that will ſoon ſtop all farther communication in theſe places likewiſe; and the fiſſures, that had been before evacuated, will be again gradually repleniſhed by the oozing of the water between the ſtrata.

58. As a ſmall quantity of vapour almoſt inſtantly generated at ſome conſiderable depth below the ſurface of the earth, will produce a vibratory motion, ſo a very large quantity (whether it be generated almoſt inſtantly, or in any ſmall portion of time) will produce a wave-like motion. The manner in which this wave-like motion will be propagated, may, in ſome meaſure, be repreſented by the following experiment. Suppoſe a large cloth, or carpet, (ſpread upon a floor) to be raiſed at one edge, and then ſuddenly brought down again to the floor, the air under it, being by this means propelled, will paſs along, till it eſcapes at the oppoſite ſide, raiſing the cloth

cloth in a wave all the way as it goes. In like manner, a large quantity of vapour may be conceived to raiſe the earth in a wave, as it paſſes along between the ſtrata, which it may eaſily ſeparate in an horizontal direction, there being, as I have ſaid before, little or no coheſion between one ſtratum and another. The part of the earth that is firſt raiſed, being bent from its natural form, will endeavour to reſtore itſelf by its elaſticity, and the parts next to it beginning to have their weight ſupported by the vapour, which will inſinuate itſelf under them, will be raiſed in their turn, till it either finds ſome vent, or is again condenſed by the cold into water, and by that means prevented from proceeding any farther.

59. If a large quantity of vapour ſhould continue to be generated for ſome time, ſeveral waves might be produced by it; and this would be, in ſome meaſure, the caſe, if the quantity at firſt generated was exceedingly great, though the whole of it was generated in leſs time, than whilſt the motion was propagated through the diſtance between two waves.

60. Theſe waves muſt riſe the higher, the nearer they are to the place from whence they have their ſource; but, at great diſtances from thence, they may riſe ſo little, and ſo ſlowly, as not to be perceived, but by the motions of waters, hanging branches in churches, *&c.*

61. The vibratory motion occaſioned by the firſt impulſe of the vapour, will be propagated through the ſolid parts of the earth, and therefore, it will much ſooner become too weak to be perceived, than the wave-like motion; for this latter, being occaſioned by the vapour inſinuating itſelf between the

ſtrata,

ſtrata, may be propagated to very great diſtances; and even after it has ceaſed to be perceived by the the ſenſes, it may ſtill diſcover itſelf by the appearances before-mentioned.

SECT. IV.

62. All earthquakes derived from the ſame ſubterraneous fire, muſt come to the ſame place in the ſame direction; and thoſe only which are derived from different fires, will come from different points of the compaſs; but as, in all probability, it ſeldom happens that earthquakes, cauſed by different fires, affect the ſame place, we therefore find in general, that they come from the ſame quarter: it is not, however, to be ſuppoſed, that this ſhould always be the caſe, for it will, probably, ſometimes happen to be otherwiſe: and this is to be expected in ſuch places as are ſituated in the neighbourhood of ſeveral ſubterraneous fires; or where, being ſubject to the ſhocks of ſome local earthquake of ſmall extent, they now and then are affected by an earthquake, produced by ſome more diſtant, but much more conſiderable cauſe. Of this laſt caſe, we ſeem to have had ſome inſtances in the earthquake of the 1ſt of November 1755, and thoſe local ones, before-mentioned, which ſucceeded it.

63. As we may reaſonably infer from many earthquakes coming to the ſame place, from the ſame point of the compaſs, that they are all derived from the ſame cauſe, and that a permanent one; ſo we may reaſonably infer the ſame thing alſo, from their being propagated with the ſame velocity; but this argument will ſtill come with the greater force, if it be conſidered,

conſidered, that the velocity of any vapour, which inſinuates itſelf between the ſtrata of the earth, depends upon the depth of it below the ſurface; for the deeper it lies, the greater will be its * velocity. We may therefore conclude, from the ſameneſs of the velocity of the earthquakes of the ſame place, that the cauſe of them lies at the ſame depth; and from the inequality of the velocity of the earthquakes of different places, that their cauſes lie at different depths. Both theſe are perfectly conſiſtent with the ſuppoſition, that earthquakes owe their origin to ſubterraneous fires, ſince the ſtrata in which theſe ſubſiſt, may be eaſily conceived to lie at different depths in different parts of the world.

SECT. V.

64. From the ſame cauſe, we may eaſily account for thoſe local earthquakes, which ſucceed the greater and more extenſive ones. If there are many ſubterraneous fires ſubſiſting in different parts of the world, the vapour coming from one fire may very well be ſuppoſed, as it paſſes, to diſturb the roof over ſome other fire, and, by that means, occaſion earthquakes by the falling in of ſome part of it: and this may be the caſe, in ſome meaſure, even where the vapour paſſes at ſome ſmall diſtance over the fire; but it will be moſt likely to take place, where the vapour either

* The velocity of ſuch a vapour, depending intirely upon the elaſticity of the earth which is over it, will be, *cæteris paribus*, (if I am not miſtaken) in the ratio of the depth below the ſurface. This ſeems to follow from a known law of all elaſtic bodies, according to which they tend to return to their ſtate of reſt, when either dilated or compreſſed, with forces proportionable to the quantity by which they differ from their natural bounds.

paſſes at ſome diſtance under it, or between the ſtratum, in which the fire lies, and that next above or below it.

PART III.

SECT. I.

65. IN the former part of this tract, I ſuppoſed a part of the roof over ſome ſubterraneous fire to fall in: this is an event that cannot happen merely accidentally; for ſo long as the roof reſts on the matter on fire, no part of it can fall in, unleſs the matter below could riſe and take its place: now, it is very difficult to conceive how this ſhould happen, unleſs it was to riſe by ſome larger paſſages than the ordinary fiſſures of the earth, which ſeem much too narrow for that purpoſe; for, beſides that the melted matter cannot be ſuppoſed to have any very great degree of fluidity, it muſt neceſſarily have a hard cruſt formed upon it, at all the fiſſures, by the long continued contact of the water contained in them: theſe impediments ſeem too great to be overcome by the difference of the ſpecific gravities of the part that is to fall in, and the melted matter, which is the only cauſe that can tend to make it deſcend; the manner therefore, in which, I ſuppoſe, this event may be brought about, is as follows:

66. The matter of which any ſubterraneous fire is compoſed, muſt be greatly * extended beyond its original

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* As all bodies we are acquainted with are liable to be extended by heat, there can be no doubt of its being ſo in this caſe likewiſe;

ginal dimenſions by the heat. As this will be brought about gradually, whilſt the matter ſpreads itſelf, or grows hotter, the parts over the fire will be gradually raiſed and bent; and this bending will, for ſome time, go on without any other conſequence; but, as the fire continues to increaſe, the earth will at laſt begin to be raiſed ſomewhat beyond the limits of it. By this means, an annular ſpace will be formed at the edges next to the fire, and ſurrounding it, a vertical ſection of which ſpace, through a diameter of the fire, will be two long triangles, the ſhorteſt ſide or baſe of each lying next the fire, and the two longer ſides being formed by the upper and lower ſtrata, which will be ſeparated for a conſiderable extent, proportionably to the diſtance through which they are raiſed from each other *. This ſpace will be gradually

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likewiſe; but the matter of ſubterraneous fires is yet much more extended, than thoſe bodies which are only capable of being melted into a ſolid glaſs, if we may judge of it from what we ſee of volcanos; for the lavas, ſciari, and pumice ſtones, thrown out from thence, even after they are cold, are commonly of much leſs ſpecific gravity, on account of their porous ſpongy texture, than the generality of earth, ſtones, &c. and they frequently are even lighter than water, which is itſelf lighter than any known foſſil bodies, that compoſe ſtrata in their natural ſtate.

* In Fig. 4. A is ſuppoſed to repreſent a vertical ſection of the matter on fire; B B, parts of the ſame ſtratum yet unkindled; C C, the two ſections of the annular ſpace, (ſurrounding the fire) which is ſuppoſed to be filled with water, as far as the ſtrata are ſeparated; D, the ſeveral ſets of earth, ſtones, &c. lying over the fire, which are raiſed a little, and bent, by the expanſion of the matter at A. As it is not eaſy to repreſent the things above deſcribed in their due proportions, it may not be amiſs, in order to prevent the figure here given from miſleading the reader, to give ſome random meaſures of the ſeveral parts, ſuch as may probably

gradually filled with water, as it is formed, the melted matter being prevented from filling it, by its want of fluidity, as well as on account of the other circumſtances, under which it is to ſpread itſelf; for the lentor and ſluggiſhneſs of this kind of matter is ſuch, that, when ſomewhat cooled on the ſurface by the contact of the air only, it will not flow, perhaps, ten feet in a month, though in a very large body; inſtances of which we have in the lavas of Ætna, Veſuvius, *&c.* It is not to be expected then, that it ſhould ſpread far, when it comes in contact with water at its edges, as ſoon as it is formed, and when it is, perhaps, ſeveral months in acquiring a thickneſs of a few inches; but it muſt, by degrees, form a kind of wall between the fire and the opening into the annular ſpace before deſcribed. This wall will gradually increaſe in height, till it becomes too tall in proportion to its thickneſs, to bear any longer the preſſure of the melted matter; which

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approach towards thoſe which are ſometimes found in nature: we may ſuppoſe then the ſtratum B to be, perhaps, from ten or twenty to a hundred yards in thickneſs; the greateſt height of the annular ſpace C, next the fire, to be from four or five to ten or fifteen feet, and its greateſt extent, horizontally, from ten or twenty to fifty or ſixty feet; the horizontal extent of the fire at A, may be from half a mile to ten or twenty miles; [See art. 29. and the note to art. 53.] and the thickneſs of the ſuperincumbent matter at D, may be from a quarter or half a mile to two or three miles; the number of the laminæ alſo, into which it is divided, may be many times more than thoſe in the figure. As to the perpendicular fiſſures, they muſt be ſo numerous, and ſo ſmall, in proportion to the other parts, that I choſe rather to leave them, to be ſupplied by the imagination of the reader, than attempt to expreſs them in a manner, that could give no adequate idea of them at all.

muſt

muſt neceſſarily happen at laſt, becauſe the thickneſs of it will not exceed a certain * limit.

67. Beſides the giving way of this wall, the fire may undermine the ſpace containing the water, and, by that means, open a communication between them. Let us ſuppoſe one of theſe come to paſs, and the time arrived when the partition begins to yield. If then the water had any way to eſcape readily, the breach would be made, and the melted matter would burſt forth immediately, and flow out in large quantities at once amongſt it; but as this is not the caſe, and it can only eſcape by oozing ſlowly between the ſtrata, and through the fiſſures, the way that it came, the breach will be made gradually, from whence we may account for ſome appearances that have preceded great earthquakes.

68. We are told, that two or three days before an † earthquake in New England, the waters of ſome wells were rendered muddy, and ſtank intolerably:

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* This limit will depend upon the thickneſs of the matter neceſſary to prevent ſo quick a communication of the heat or cold through it, as that the water ſhould be able to diminiſh the heat of the fire conſiderably. The thickneſs requiſite to do this, is very different in different kinds of bodies. Metals of all kinds tranſmit heat and cold extremely readily; but bricks and vitrified ſubſtances (with which laſt we may claſs the matter under our preſent conſideration) tranſmit them very ſlowly: the walls of the hotteſt of our furnaces, when built of bricks, and eighteen inches thick, will not tranſmit more heat than a living animal can bear without injury, though the fires are continued in them for ever ſo long a time; probably, therefore, if we allow two feet for the thickneſs of the matter, cooled and rendered hard by the contact of the water, we ſhall not underdo it.

† See Philoſ. Tranſ. N° 437. or Martyn's Abridgm. vol. viii. p. 689.

why might not this be occaſioned by the waters contained in the ſpaces before deſcribed, which, being impregnated with ſulphureous ſteams, were driven up, and mixed with the waters of the ſprings? At leaſt, there can be no doubt, by whatſoever means it was brought about, that this phænomenon was owing to the ſame cauſe, already beginning to exert itſelf, which afterwards gave riſe to the ſucceeding earthquake.

69. Something like this happened before the great Liſbon * earthquake of 1755. We are told, that at Colares, about twenty miles from thence, " in the afternoon preceding the 1ſt of No-
" vember, the water of a fountain was greatly de-
" creaſed: on the morning of the 1ſt of November,
" it ran very muddy, and after the earthquake, it re-
" turned to its uſual ſtate, both in quantity and clear-
" neſs." The ſame author ſays, a little lower, " in
" the afternoon of the 24th, I was much apprehen-
" ſive, that the following days we ſhould have an-
" other great earthquake; for I obſerved the ſame
" prognoſtics as in the afternoon of the 31ſt October;
" that is," &c. " And I farther obſerved, that the
" water of a fountain began to be diſturbed to ſuch
" a degree, that in the night it ran of a yellow clay
" colour; and from midnight to the morning of the
" 25th, I felt five ſhocks, one of which ſeemed to
" me as violent as that of the 11th of December."

70. But the moſt extraordinary appearance of any that preceded this earthquake, was that of the agita-

* See Philoſ. Tranſ. vol. xlix, p. 416 and 417.—or Hiſt. and Philoſ. of Earthq. p. 313.

tion

tion of the waters of * Lochneſs, and ſome others of the lochs in Scotland, about half an hour before any motion was felt at Liſbon, notwithſtanding the cauſe of all theſe great effects could not lie far from thence, and, I think, certainly lay to the ſouth of Oporto. Nor is it probable, that there ſhould be any miſtake in the time, not only becauſe the difference is too great, as well as the concurrent teſtimonies too many, to admit of ſuch a ſolution; but becauſe they mention another greater agitation, that happened about an hour and half after the former; which latter agrees with the times, when the agitations of the waters were obſerved in England, if we allow only a proper interval for the motion to be propagated ſo far northward, proportionably to the time it took up in travelling from its original ſource near Liſbon.

71. Theſe appearances ſeem to be connected with that mentioned in the preceding article, and they may both, I think, be accounted for, by ſuppoſing a conſiderable quantity of vapour to be raiſed, whilſt the partition before-mentioned was beginning to give way; during which time, a partial

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* See Philoſ. Tranſ. vol. xlix.—or Hiſt. and Philoſ. of Earthq. art. Lochneſs, Lochlommond, *&c.* The ſame thing alſo ſeems to have taken place in Switzerland; for Monſ. Bertrand ſays, that all the agitations of the waters in the lakes there, which were obſerved on the 1ſt November 1755, happened between nine and ten in the morning; and particularly at lake Leman, he ſays, the agitation happened juſt before ten; which, allowing for the difference of longitude, muſt have been juſt before nine at Liſbon; and, conſequently, if there is no miſtake in the times, all theſe agitations preceded the earthquake, at this laſt place, by near three quarters of an hour. [See *Memoires ſur les tremblemens de Terre, p.* 107 *et* 105.]

communi-

communication between the water and fire would be brought on, and that by degrees only. Hence the vapour, not being produced at once but gradually, might creep * ſilently between the ſtrata, towards that quarter where the ſuperincumbent maſs of earth was lighteſt; and, by this means, ſome places very near the ſource of the vapour might be little, or not at all, affected by it, whilſt others might be greatly affected, though they lay at a great diſtance; and even thoſe places, which lay immediately over the part where the vapour was paſſing, might not perceive any effect, on account of the gentleneſs of the motion, occaſioned by the ſmall quantity of it. This might continue to be the caſe, till it came to ſome country where, the ſet of ſtrata above being much thinner, the vapour would not only be hurried forward, but collected alſo into a much narrower compaſs; and therefore, raiſing the earth more, would produce more ſenſible effects; and this we ought

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* Some appearances that have been obſerved in New England ſeem to confirm this, and make it probable, that a ſmall quantity of vapour is often found to creep ſilently between the ſtrata, before a general communication between the water and the fire gives riſe to the greater and more ſenſible effects of earthquakes. See Philoſ. Tranſ. N° 462. or Martyn's Abr. vol. viii. p. 693. where we are told, that, at Newbury, a little before any noiſe or ſhock was perceived, the bricks of an hearth were obſerved to riſe, and, falling down again, to lean another way. In the ſame account, it is alſo ſaid, that "a few minutes before any ſhock came, many people "could foretell it by an alteration in their ſtomachs:" an effect, which ſeems to be of the ſame kind with ſea-ſickneſs, and which always accompanies the wave-like motion of earthquakes, when it is ſo weak, as to be uncertainly diſtinguiſhable.

chiefly

chiefly to expect in the moſt mountainous countries, according to the idea before given of them *.

72. To make this ſomething clearer, let us ſuppoſe, in Fig. 1. the vapour to be paſſing between the ſtrata in the dotted line C, and to go forwards, till it arrives at A: whilſt, then, it paſſes under the deeper parts at E, it will raiſe the earth over it but little, as well becauſe it will be ſpread broader and thinner, as becauſe it will be more compreſſed by the weight of the ſuperincumbent matter; but as it arrives towards A, not only the latter part will be driven forwards with greater velocity, but the foremoſt will travel ſlower, on account of its travelling under a † thinner ſet of ſtrata; and, beſides this, the load being much leſs, it will greatly expand itſelf. From all theſe cauſes taken together, the wave at the ſurface of the earth, occaſioned by the paſſing of the vapour under it, will not only be much higher, but alſo much ſhorter, and, conſequently, the ſides of it, on both theſe accounts, will be much more inclined to the horizon: and, moreover, becauſe the progreſs of the wave will be ſlower, it will give more time to any waters ſituated on one ſide of it, to flow one way; and on this account alſo, the apparent agitation of them will be increaſed.

SECT. II.

73. We are told, that, in the Liſbon earthquake of 1755, "the bar [at the mouth of the Tagus] was "ſeen dry from ſhore to ſhore; then ſuddenly the ſea,

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* See art. 43.

† See art. 63. the note.

" like a mountain, came rolling in; and about Bel-
" lem caſtle, the water roſe fifty feet almoſt in an
" inſtant; and, had it not been for the great bay
" oppoſite to the city, which received and ſpread
" the great flux, the low part of it muſt have been
" under water *." The ſame phænomena were obſerved to accompany the ſame earthquake at the iſland of Madeira; where we are told, that, at the city of Funchal, " the ſea, which was quite calm, was ob-
" ſerved to retire ſuddenly ſome paces; then riſing
" with a great ſwell, without the leaſt noiſe, and as
" ſuddenly advancing, it overflowed the ſhore, and
" entered the city. It roſe full fifteen feet perpen-
" dicular above high-water mark, although the tide,
" which ebbs and flows there ſeven feet, was then
" at half ebb. In the northern part of the iſland,
" the inundation was more violent, the ſea retiring
" there above one hundred paces at firſt, and ſud-
" denly returning, overflowed the ſhore, forcing
" open doors, breaking down the walls of ſeveral
" magazines and ſtorehouſes, and carrying away, in
" its receſs, a conſiderable quantity of grain, and
" ſome hundred pipes of wine †."

74. Both theſe appearances (which have been obſerved to attend ſeveral other earthquakes, as well as this) ſeem to admit of an eaſy ſolution, ſuppoſing the cauſe of them to lie under the bed of the ocean; for, in the farther progreſs of the communication between the fire and water, the vapour, that is

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* See Hiſt. and Philoſ. of Earthq. p. 316.

† See Philoſ. Tranſ. vol. xlix. p. 432, &c.—or Hiſt. and Philoſ. of Earthq. p. 329.

gradually raiſed at firſt, will at laſt begin to raiſe the roof over the fire, which, being ſupported by ſo light a vapour, there will now be no want of fluidity in the matter it reſts upon, and the difference of ſpecific gravity between the two, inſtead of being ſmall, will be very great: hence, if any part of the roof gives way, it muſt immediately fall in, the vapour readily riſing, and taking its place; and a beginning being once made, a communication will be opened with numberleſs clefts and fiſſures, that muſt occaſion the falling in of vaſt quantities of matter, which, as ſoon as the vapour can paſs round them, will want their ſupport; then will follow the great * effects already deſcribed.

75. Now, whilſt the roof is raiſing, the waters of the ocean, lying over it, muſt retreat, and flow from thence every way; this, however, being brought about ſlowly, they will have time to retreat ſo gently, as to occaſion no great diſturbance: but as ſoon as ſome part of the roof falls in, the cold water contained in the fiſſures of it, mixing with the ſteam, will immediately produce a vacuum, in the ſame manner as the water injected into the cylinder of a ſteam engine, and the earth ſubſiding, and leaving a hollow place above, the waters will flow every way towards it, and cauſe a retreat of the ſea on all the ſhores round about: then preſently, the waters being again converted by the contact of the fire into vapour, together with all the additional quantity, which has now an open communication with it, the earth will be raiſed, and the waters over it will be made to flow every

* See art. 56 to 60 incluſive.

way, and produce a great wave immediately ſucceeding the previous retreat *.

SECT. III.

76. That great quantity of water, which we have ſuppoſed to be let out upon ſubterraneous fires, and, by that means, to produce earthquakes, will ſupply us with a reaſon, why they obſerve a ſort of periodical return. This water muſt extinguiſh a great portion of the burning matter, in conſequence of which, it will be contracted within much narrower bounds; and though the effects before deſcribed could not take place at firſt, but by the great extenſion of the heated matter, yet, after they have once taken place,

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* It may, perhaps, be objected, that theſe phænomena may as eaſily be occaſioned by a vapour generated under the dry land, which, by firſt raiſing the earth upon the ſea-ſhore, would make the waters retreat; and that the return of them again, upon its ſubſiding into its place, might cauſe the ſubſequent wave. That this may be the caſe, in ſome inſtances, is not impoſſible, but, I believe, upon examining the particular circumſtances, it will generally be found to be otherwiſe; and there cannot be any doubt about it, in the caſe of the Liſbon earthquake; for the retreat was obſerved to precede the wave, not only on the coaſt of Portugal, but alſo at the iſland of Madeira, and ſeveral other places: now, if the retreat had been cauſed by the raiſing of the earth on the coaſt of Portugal, the motion of the waters occaſioned by this means, when propagated to Madeira, muſt have produced a wave there previous to the retreat, contrary to what happened; nor could the motion of the waters at Madeira be cauſed by the earthquake at that place, becauſe it did not happen till above two hours after; whence it is manifeſt, that it muſt have been owing to the continuation of a motion propagated from the place, where the earthquake exerted its firſt efforts. And we may obſerve, in general, that this muſt always be the caſe, whenever the retreat does not happen till ſome conſiderable time after the earthquake.

they

they may well continue to do ſo for ſome time; for the great diſturbance in the firſt inſtance, by the falling in of a great part of the roof, muſt render the frequent communication between the fire and water not only very eaſy, but almoſt unavoidable: and this will continue to be ſo, till the roof is well ſettled, and the ſurface of the melted matter ſufficiently cooled, after which, it may require a long time for the fire to heat it again ſo much, as will be neceſſary to make it produce the former effects. Now, as the matter has been more or leſs cooled, or as the combuſtible materials are with more or leſs difficulty ſet on fire again, as well as on account of other circumſtances, the returns of theſe effects will be later or earlier; but though they will not, for this reaſon, obſerve any exact period, yet they will generally fall within ſome ſort of limits, till either the matter that occaſions them is conſumed, (which, probably, will ſeldom happen in leſs than many ages) or till the fires open themſelves a paſſage, and become volcanos.

SECT. IV.

77. I have already intimated, that the moſt extenſive earthquakes frequently take their riſe from the ſea. According to the deſcription of the * ſtructure of the earth before given, any combuſtible ſtratum muſt lie at greater depths in places under the ocean, than elſewhere; hence far more extenſive fires may ſubſiſt there, than where the quantity of matter over them is leſs; for any vapour raiſed from ſuch fires,

* See art. 43.

having

having both a ſtronger roof over it, and being preſſed by a greater weight, (beſide the additional weight of the water) will not only be leſs at liberty to expand itſelf, and conſequently of leſs bulk, but it will alſo be eaſily driven away towards the parts round about, where the ſuperincumbent matter is leſs, and therefore lighter. On the other hand, any vapour raiſed from fires, where the ſuperincumbent matter is lighter, finding a weaker roof over it, and being not ſo eaſily driven away under ſtrata, that are thicker and heavier, will be very apt to break through, and open a mouth to a volcano; and it muſt neceſſarily do this long before the fires can have ſpread themſelves ſufficiently, to be near equal to thoſe which may ſubſiſt in places that lie deeper. All this ſeems to be greatly confirmed by the ſituation of volcanos, which are almoſt always found on the * tops of mountains, and thoſe often ſome of the higheſt in the world.

78. If, then, the largeſt fires are to be ſuppoſed to ſubſiſt under the ocean, it is no wonder that the

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* Perhaps this may ſupply us with a hint (if the conjecture is not thought extravagant) concerning the manner in which theſe mountains have been raiſed, and why the ſtrata lie generally more inclining from the mountainous countries, than thoſe countries themſelves; an appearance not eaſily to be accounted for, but upon the ſuppoſition, that the upper parts of the earth reſt upon matter, in ſome degree, though not perfectly fluid, and that this matter is lighter than the earth that reſts upon it. This conjecture, however, will probably be thought leſs ſtrange, if it be conſidered, that the new iſlands, formed about Santerini and the Azores, have ſome of them been raiſed from 200 to 300 yards, and upwards; a height which might well enough intitle them to the denomination of mountains, if they had been raiſed from lands not lying under the ocean. [See Fig. 3.]

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moſt extenſive earthquakes ſhould take their riſe from thence: the great earthquake of Liſbon has been * ſhewn to have done ſo; and that the cauſe of it was alſo at a greater depth, than that of many others, appears from the greater † velocity with which it was propagated.

79. The great earthquake that deſtroyed Lima and Callao in 1746, ſeems alſo to have come from the ſea; for ſeveral of the ports upon the coaſt were overwhelmed by a great wave, which did not arrive till four or five minutes after the earthquake began, and which was preceded by a ‡ retreat of the waters, as well as that at Liſbon. Againſt this, it may, perhaps, be alleged, that there were four ‖ volcanos broke out ſuddenly, in the neighbouring mountains, when this earthquake happened, and that the fires of theſe might be the occaſion of it. This however, I think, is not very probable; for, to omit the argument of the wave, and previous retreat of the waters, already mentioned, it is not very likely, that more than one fire was concerned: beſides, the vapour, opening itſelf a paſſage at theſe places, could not well be ſuppoſed, if it took its riſe from thence, to ſpread itſelf far; eſpecially towards the ſea, where it is manifeſt,

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* See art. 54. See alſo art. 94 to 97 incluſive.

† See the note to art. 63.

‡ Both the wave and previous retreat have been obſerved in the other great earthquakes, which have happened at Lima, and in the neighbouring country. See d'Ulloa's Voyage to Peru, part ii. book i. chap. 7.

‖ If theſe volcanos were not new ones, but only old ones which broke out afreſh, [See the note to art. 34.] the argument will come with ſtill greater force.

that

that the ſtrata over it were of great thickneſs, as appears from the great velocity with which the earthquake was propagated there: the ſhocks alſo continued with equal, or nearly equal violence, for ſome months after the openings were made; whereas, if theſe fires had been the cauſe of them, they muſt immediately have ceaſed, upon the fires finding a vent, as it has happened in other * caſes. It is therefore much more probable, that a very large quantity of vapour, taking its riſe from ſome far more extenſive fire under the ſea, ſpread itſelf from thence; and as it paſſed in places, where the roof over it was naturally much thinner, as well as greatly weakened by the undermining of theſe fires, it opened itſelf a paſſage, and burſt forth.

80. As the moſt extenſive earthquakes generally proceed from the loweſt countries, but eſpecially from the ſea, ſo thoſe of a ſmaller extent are generally found amongſt the mountains: hence it almoſt always happens, that earthquakes, which are felt near the ſea, if at all violent, are felt alſo in the higher lands; whereas there are many amongſt the hills, and thoſe very violent ones, which never extend themſelves to the lower countries. Thus we are told, that, at Jamaica, " † ſhakes often happen in " the country, not felt at Port-Royal; and ſome- " times are felt by thoſe that live in and at the foot

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* See art. 28.

† This is taken from an account of the earthquake that happened at Jamaica in the year 1692, which, as well as ſome others before-mentioned, was attended with the wave and previous retreat. See Philoſ. Tranſ. N° 209. or Lowthorp's Abr. vol. ii. p. 417 and 418.

" of

" of the mountains, and by no body elſe." On the other hand, the earthquake that deſtroyed Port-Royal extended itſelf all over the iſland: and the ſame was obſerved of a ſmaller earthquake, that happened there in 1687-8; which latter undoubtedly came from the ſea, as appears by Sir * Hans Sloane's account of it.

81. Earthquakes of ſmall extent are alſo very common amongſt the mountains of Peru and Chili. Antonio d'Ulloa ſays, " Whilſt we were preparing for " our departure from the mountain Chichi-Choco, " there was an earthquake which was felt four " leagues round about: our field tent was toſſed to " and fro by it, and the earth had a motion like " that of waves; this earthquake, however, was " one of the ſmalleſt, that commonly happen in that " country." The ſame author tells us, in another place, that, " during his ſtay at the city of Quito, " or in the neighbourhood of it, there were two " earthquakes, violent enough to overturn ſome " houſes in the country, which buried ſeveral perſons " under their ruins."

Sect. V.

82. It is generally found, that earthquakes in hilly countries, are much more violent than thoſe, which happen elſewhere; and this is obſerved to be the caſe, as well when they take their riſe from the lower countries, as amongſt the hills themſelves. This appearance being ſo eaſily to be accounted for, from the ſtructure of the earth already deſcribed, I

* See Phil. Tranſ. N° 209, or Lowthorp's Abr. vol. ii. p. 410.

ſhall content myſelf with eſtabliſhing the certainty of a fact, which tends ſo greatly to confirm it.

83. The earthquakes that have infeſted ſome of the towns in the neighbourhood of Quito, have not only been incomparably more violent than that which deſtroyed Liſbon, but they ſeem to have exceeded that alſo which deſtroyed Lima and Callao. In * Liſbon, many of the houſes were left ſtanding, although few of them were leſs than four or five ſtories high. At Lima alſo, it is only ſaid, that "all "the buildings, great and ſmall, or at leaſt the "greateſt part of them, were deſtroyed." Callao likewiſe, as it appears from the accounts we have of it, had many houſes left unhurt by the earthquake, till the wave came, which overwhelmed the whole town, and threw down every thing that lay in its way. All theſe effects ſeem to be greatly ſhort of thoſe produced by an earthquake that happened at Latacunga, in the year 1698, when the whole town, conſiſting of more than ſix hundred houſes, was entirely deſtroyed in leſs than three minutes time, a part of one only eſcaping; notwithſtanding that the houſes there are never built more than one ſtory high, in order, if poſſible, to avoid theſe dangers. Ambato, a village about the ſame ſize as Latacunga, together with a great part of Riobamba, another town in the ſame neighbourhood, were alſo entirely deſtroyed by the ſame earthquake, and ſome others were either deſtroyed, or received conſiderable damage

* See Philoſ. Tranſ. vol. xlix. p. 403. where it is ſaid, "of "the dwelling-houſes, there might be about one fourth of them "that tumbled."

from

from it. At the ſame time, a volcano burſt out ſuddenly in the neighbouring mountain of Carguayraſo, as before-mentioned; and, "near Ambato, the earth "opened itſelf in ſeveral places, and there yet re-"mains, to the ſouth of that town, a cleft of four "or five feet broad, and about a league in length, "lying north and ſouth; there are alſo ſeveral other "like clefts on the other ſide of the river." The city of * Quito was affected at the ſame time, but received no damage, though it is no more than forty-two geographical miles from Latacunga, not far from whence the greateſt violence of the ſhock ſeems to have exerted itſelf. Theſe towns are ſuppoſed to ſtand by far the higheſt of any in the world, being as high above the level of the ſea, as the tops of ſome of the higheſt mountains in Europe; and the ground upon which Riobamba ſtands, wants but † ninety yards of being three times as high as Snowdon, the higheſt mountain in Wales.

84. The country upon which theſe towns ſtand, ſerves as a baſe, from whence ariſe another ſet of high lands and mountains, which are much the higheſt in the known world. Amongſt theſe mountains there are no leſs than ſix volcanos, if not more,

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* The city of Quito ſtands lower than the level of Riobamba, by about 500 yards perpendicular. Though it eſcaped this, it has lately, however, been deſtroyed by another violent earthquake, that happened on the 28th April 1756, of which I have not yet ſeen any other particulars worth notice.

† This is according to Antonio d'Ulloa's account; but Monſ. Condamine makes it exactly three times the height of Snowdon, computing it at 1770 toiſes. [See his meaſure of a degree of the meridian.]

within an extent of 120 miles long, and leſs than thirty broad, the loweſt of which exceeds the height of Riobamba by above two thirds of a mile, and the higheſt by more than twice that quantity. Now, as the earthquakes have been more violent at the foot of theſe mountains, than in the lower lands, ſo they have been ſtill more violent towards the tops of them: this is ſufficiently manifeſt, from the many * rents made in them, and the rocks that have been broken off from them, upon ſuch occaſions: but it appears ſtill more manifeſtly, and beyond all diſpute, in the burſting forth of volcanos, which are almoſt always at the very † ſummit of the mountains, where they are found. In theſe inſtances, the earth, ſtones, *&c.* which lay over the fire, are generally ſcattered by the violence of the vapour, that breaks its way through, to the diſtance of ſome miles round about.

85. The great earthquake of the 1ſt November 1755, was alſo more violent amongſt the mountains, than at the city of Liſbon. We are told, that "the "mountains of Arrabida, Eſtrella, Julio, Marvan, "and Cintra, being ſome of the largeſt in Portugal, "were impetuouſly ſhaken, as it were, from their "very foundations; and moſt of them opened at "their ſummits, ſplit and rent in a wonderful man-"ner, and huge maſſes of them were thrown down "into the ſubjacent vallies ‡."

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* See d'Ulloa's Voyage to Peru, part i. book vi. chap. 2.

† The only exceptions that I know of to this rule, are in thoſe caſes, where the higheſt part having an opening already, ſome freſh mouth opens itſelf in the ſide of the mountain.

‡ See Hiſt. and Philoſ. of Earthq. p. 317.

86. The

86. The ſame was obſerved at Jamaica likewiſe. In the earthquake that deſtroyed Port-Royal in 1692, we are told, that " more houſes were left ſtanding " at that town, than in all the iſland beſides. It was " ſo violent in other places, that people were violently " thrown down on the ground, where they lay with " their legs and arms ſpread out, to prevent being " tumbled about by the incredible motion of the " earth. It ſcarce left a planter's houſe or ſugar- " work ſtanding all over the iſland: I think it left " not a houſe ſtanding at Paſſage fort, and but one " in all Liganee, and none in St. Iago, except a few " low houſes, built by the wary Spaniards. In Cla- " rendon precinct, the earth gaped, and ſpouted up, " with a prodigious force, great quantities of water " into the air, twelve miles from the ſea; and all " over the iſland, there were abundance of openings " of the earth, many thouſands. But in the moun- " tains, are ſaid to be the moſt violent ſhakes of all; " and it is a generally received opinion, that the " nearer to the mountains, the greater the ſhake; " and that the cauſe thereof, whatever it is, lies " there. Indeed they are ſtrangely torn and rent, " eſpecially the blue, and other higheſt mountains, " which ſeem to be the greateſt ſufferers, and which, " during the time that the great ſhakes continued, " bellowed out prodigious loud noiſes and echo- " ings.

87. " Not far from Yallowes, a mountain, after " having made ſeveral moves, overwhelmed a whole " family, and a great part of a plantation, lying a " mile off; and a large high mountain near Port-

" morant,

" morant, near a day's journey over, is ſaid to be " quite ſwallowed up.

88. " In the blue mountains, from whence came " thoſe dreadful roarings, may reaſonably be ſup- " poſed to be many ſtrange alterations of the like " nature; but thoſe wild deſart places being very " rarely, or never viſited by any body, we are yet " ignorant of what happened there; but whereas " they uſed to afford a fine green proſpect, now one " half part of them, at leaſt, ſeem to be wholly de- " prived of their natural verdure *."

SECT. VI.

89. I have ſuppoſed, that fires lying at the greateſt depths generally produce the moſt extenſive earthquakes, we muſt, however, except from this rule thoſe caſes where the depths are very great; for, as the weight of three miles perpendicular of common earth is capable of abſolutely repreſſing the vapour of inflamed gunpowder, ſo we may well ſuppoſe, that

* See Philoſ. Tranſ. N° 209. or Lowthorp's Abridg. vol. ii. p. 416, &c. where there is a great deal more to the ſame purpoſe. See alſo Hiſt. and Philoſ. of Earthq. p. 286 and 287.

From the authorities quoted in this ſection, it appears, how little reaſon there is for the notion, that either large cities, or towns ſituated near the ſea-coaſt, are more ſubject to violent earthquakes than others: it is not, however, much to be wondered at, that ſuch a notion ſhould have prevailed, after the great deſtruction that happened in ſo large and populous a city as Liſbon; ſince the demolition of a few ruinous houſes only, in ſuch a place, would have affected the imaginations of men more, and would have been more talked of, than the ſubverſion of whole mountains in ſome wild and deſart country, where at moſt half a dozen unknown ſhepherds might feel the effects of it, or perhaps only ſee it at a diſtance.

there

there may be a quantity of earth ſufficient to repreſs the vapour of water, and keep it within its original limits, though ever ſo much heated. Now, whenever this is the caſe, it is manifeſt, that it can produce no effect: or, it may happen, that though the quantity of earth may not be ſufficient abſolutely to repreſs the vapour, yet it may be ſo great, as to ſuffer it to expand but very little: in this caſe, an earthquake ariſing from it would be but of ſmall extent; the wave-like motion would be little or none; the vibratory motion would be felt every-where; and the propagation of the motion would be very quick. This laſt circumſtance being almoſt the only one, by which theſe earthquakes can be known from thoſe which owe their origin to ſhallower fires, it muſt be very difficult to diſtinguiſh them with certainty, as it is almoſt impoſſible to diſtinguiſh the difference of the time of their happening in different places, when the whole, perhaps, is comprehended within the ſpace of two or three minutes; poſſibly, however, ſome of the earthquakes, which we have had in England, may have been of this claſs.

SECT. VII.

90. If we would inquire into the place of the origin of any particular earthquake, we have the following grounds to go upon.

91. *Firſt*, The different directions, in which it arrives at ſeveral diſtant places: if lines be drawn in theſe directions, the place of their common interſection muſt be nearly the place ſought: but this is liable to great difficulties; for there muſt neceſſarily be great uncertainty in obſervations, which cannot, at beſt,

beſt, be made with any great preciſion, and which are generally made by minds too little at eaſe to be nice obſervers of what paſſes; moreover, the directions themſelves may be ſomewhat varied, by the inequalities in the weight of the ſuperincumbent matter, under which the vapour paſſes, as well as by other cauſes.

92. *Secondly*, We may form ſome judgment concerning the place of the origin of a particular earthquake, from the time of its arrival at different places; but this alſo is liable to great difficulties. In both theſe methods, however, we may come to a much greater degree of exactneſs, by taking a medium amongſt a variety of accounts, as they are related by different obſervers. But,

93. *Thirdly*, We may come to the greateſt degree of exactneſs in thoſe caſes, where earthquakes have their ſource from under the ocean; for, in theſe inſtances, the proportional diſtance of different places from that ſource may be very nearly aſcertained, by the interval between the earthquake and the ſucceeding wave: and this is the more to be depended on, as people are much leſs likely to be miſtaken in determining the time between two events, which follow one another at a ſmall interval, than in obſerving the preciſe time of the happening of ſome ſingle event.

94. Let us now, by way of example, endeavour to inquire into the ſituation of the cauſe, that gave riſe to the earthquake of the 1ſt of November 1755, the place of which ſeems to have been under the ocean, ſomewhere between the latitudes of Liſbon and Oporto, (though probably ſomewhat nearer to the

the former) and at the diſtance, perhaps, of ten or fifteen leagues from the coaſt. For,

95. *Firſt*, The direction, in which the earthquake arrived at Liſbon, was from the north-weſt; at Madeira it came from the north-eaſt; and in England it came from the ſouth-weſt; all of which perfectly agree with the place aſſumed *.

96. *Secondly*, The times in which the earthquake arrived at different places, agree perfectly well alſo with the ſame point. And,

97. *Thirdly*, The interval between theſe, and the time of the arrival of the ſubſequent wave, concur in confirming it. That all this might appear the better, I have ſubjoined the following table, aſſuming the point, from whence I compute, at the diſtance of about a degree of a great circle from Liſbon, and a degree and half from Oporto. In conſequence of this ſuppoſition, I have added three minutes to the interval between the time when the ſhock was felt at Liſbon, and at the ſeveral other places. The firſt column in the table contains the names of places; the ſecond, the diſtances from the aſſumed point, reckoned in half degrees; the third, the time that the earthquake took up in travelling to each, expreſſed in minutes; and the fourth contains the time in which the wave was propagated, from its ſource to the reſpective places, expreſſed in minutes likewiſe.

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* All theſe directions, together with the times when the earthquake, as well as the ſucceeding wave, arrived at different places, (two or three only excepted) are taken from the 49th volume of the Philoſ. Tranſ. and the Hiſt. and Philoſ. of Earthq. To theſe, I muſt refer the reader for the particular authorities, which, as they are very numerous, I was not willing to quote at length.

	Half deg.	Min.	Min.
Liſbon * - - - - - - -	2	3	12
Oporto * - - - - - -	3	5	
Ayamonte - - - - - -	6		53
Cadiz - - - - - - -	9	12	82
Madrid - - - - - - -	9	11	
Gibraltar - - - - - -	11	18	
Madeira - - - - - - -	19	25	152
Mountſbay - - - - - -	20		267
Plymouth - - - - - -	21		360
Portſmouth - - - - - -	23	29	
Kingſale - - - - - -	23		290
Swanſea - - - - - -	24		530
The Hague - - - - - -	30	32	
Lochneſs - - - - - -	33	66	
Antigua - - - - - - -	98		565
Barbadoes - - - - - -	101		485

98. In computing the times in the preceding table, allowance was made for the difference of longitude, as it is laid down in the common maps, which are

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* It appears, by all the accounts, that the interval between the earthquake and wave, either at Oporto or Liſbon, was not long: I have met with no account yet, however, which tells us how long it was at the former, and only one which mentions it at the latter, where it is ſaid to have been nine minutes. [See *Memoires ſur les tremblemens de Terre*, *p.* 245. compared with Hiſt. of Earthquakes, p. 315.] Theſe intervals, if we knew them exactly, might have ſerved, perhaps, to aſcertain the diſtance of thoſe two places from the original ſource a little more accurately; but, as the diſtance of neither from thence could be very great, a ſmall difference in them would hardly ſenſibly affect any of the others; from which, therefore, we may draw the ſame general concluſions, as if they were exact.

not

not always greatly to be depended on. The times themſelves alſo are often ſo careleſly obſerved, as well as vaguely related, that they are many of them ſubject to conſiderable errors; the concurrent teſtimonies, however, are ſo many, that there can be no doubt about the main point; and, that the errors might be as ſmall as poſſible, I have not only endeavoured to ſelect thoſe accounts that had the greateſt appearance of accuracy, but, in all caſes where it was to be had, I have always taken a mean amongſt them. In many of the accounts, the relaters ſay only between ſuch hours, or about ſuch an hour: of this kind were the accounts of the times of the agitation of the waters at The Hague and Lochneſs, which vary the moſt from a medium of the reſt, the former erring about ſeven minutes in defect, and the latter about twenty minutes in exceſs; with regard to the latter, however, I muſt obſerve, that, from the account itſelf, it is probable the agitation happened ſooner than eleven o'clock, which is the time mentioned. The accounts alſo of the time of the agitation of the waters in the northern parts of England, ſeem to confirm the ſame thing *.

99. It is obſervable, in the preceding table, that the times, which the wave took up in travelling, are

* As the ſhorteſt way that the vapour could paſs from near Liſbon to Lochneſs was under the ocean, poſſibly it might, on that account, be ſomewhat retarded; for the water adding to the weight of the ſuperincumbent maſs, and not to its elaſticity, muſt produce this effect in ſome degree; it is probable, however, that this could make no great difference, as the motion ſeems to have been very little retarded in its paſſage from the original ſource to Madeira, to which place, I ſuppoſe, it muſt have paſſed under deeper ſeas than would be found in its road to Scotland.

not in the ſame proportion with the diſtances of the reſpective places from the ſuppoſed ſource of the motion; this, however, is no objection againſt the point aſſumed, ſince it is manifeſt, wherever it was, that it could not be far from Liſbon, as well becauſe the wave arrived there ſo very ſoon after the earthquake, as becauſe it was ſo great, riſing, as we are told, at the diſtance of three miles from Liſbon, to the height of fifty or ſixty feet. The true reaſon of this diſproportion, ſeems to be the difference in the depth of the water; for, in every inſtance in the above table, the time will be found to be proportionably ſhorter or longer, as the water through which the wave paſſed was * deeper or ſhallower. Thus the motion of the wave to Kingſale or Mountſbay (through waters not deeper in general than 200 fathoms) was ſlower than that to Madeira, (where the waters are much deeper) in the proportion of about three to five; and it was ſlower than that to Barbadoes, (where its courſe lay through the deepeſt part of the Atlantic ocean) nearly in the proportion of one to three: ſo likewiſe the motion of it from the Scilly iſlands to Swanſea in Wales (where the depth gradually diminiſhes from about ſixty or ſeventy fathoms to a very ſmall matter) was ſtill ſlower than that to Kingſale, in the proportion of leſs than one to three: the ſame thing is obſervable with regard to

* We have an inſtance to this purpoſe in the tides, which, in deep waters, move with a velocity that would carry them round the whole earth in a ſingle day; but as they get into ſhallower waters, they are greatly retarded: and we are told, that in the river of Amazons, the ſame tide is found running up to the tenth or twelfth day, before it is entirely ſpent. [See *Condamine's Voyage down the Maranon.*]

Plymouth

Plymouth alſo, where the wave arrived about ninety minutes later than at Mountſbay, though the difference of their diſtance from the firſt ſource could not, upon any ſuppoſition, be more than forty or fifty miles.

SECT. VIII.

100. If we would inquire into the depth, at which the cauſe lies, that occaſions any particular earthquake, I know of no method of determining it, which does not require obſervations not yet to be had; but if ſuch could be procured, and they were made with ſufficient accuracy, I think ſome kind of gueſs might be formed concerning it: for,

101. *Firſt*, In thoſe inſtances, where the vapour diſcharges itſelf at the mouths of volcanos, (as in the caſe of the earthquake at Lima) it might, perhaps, be poſſible for a careful obſerver to trace the * thickneſs of the ſeveral ſtrata from thence to the place where the earthquake took its riſe, or at leaſt as far as the ſhore, if it took its riſe from under the ſea. If this could be once done in any one inſtance, and the velocity of ſuch an earthquake nicely determined, we might then gueſs at the depth of the cauſe in other earthquakes, where we knew their velocity, by taking the † depths proportional to thoſe velocities, which probably would anſwer very nearly.

102. *Secondly*, If, in any inſtance, it ſhould be poſſible to know how much the motion of any earthquake was retarded by paſſing under the ocean, the

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* This is upon the ſuppoſition, that the under ſtrata, in aſcending up the hills, come to the day in the manner before deſcribed. See art. 43. and Fig. 3.

† See the note to art. 63.

depth

depth of the ocean being known, the depth at which the vapour paſſed would be known alſo; for the velocity under the water would be to the velocity, if there had been no water, in the ſubduplicate ratio of the weight in the latter caſe to the weight in the former; hence allowing earth to be about two and half times the weight of water, the depth will be readily found.

103. *Thirdly*, Let us conceive the earth to be formed according to the idea before given of it, and that the ſame ſtrata are at a medium of the ſame thickneſs for a very great extent, as well in thoſe places, where ſeveral of the upper ones are wanting, as where they are not. Upon this ſuppoſition, we may diſcover the depth, at which the vapour paſſes, by comparing the ſeveral velocities of the ſame earthquake in places, where the * thickneſſes of the ſuperincumbent maſs are different. It muſt be acknowleged, indeed, that ſuch obſervations with regard to time, as would enable us to determine theſe velocities, are in general much too nice to be expected: the matter, however, is not altogether deſperate, as we may collect them, in ſome meaſure perhaps, from other circumſtances, ſuch, for inſtance, as the degree of † agitation in different waters, the proportional ‡ ſuddenneſs, with which the earth is lifted in different places, *&c.*

104. As

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* In order to know this difference, it will be neceſſary to trace the thickneſs of thoſe ſtrata, which are found in ſome of the places, but are wanting in others.

† See art. 71 and 72.

‡ This may be known from the diſtance to which the mercury ſubſides in the barometer, upon the firſt raiſing of the earth by the vapour.

104. As the obſervations relating to the earthquake of the 1ſt of November 1755 are too groſs, it would be in vain to attempt, by any of the foregoing methods, to determine with any certainty the depth at which the cauſe of it lay; but, if I might be allowed to form a random gueſs about it, I ſhould ſuppoſe, (upon a compariſon of all circumſtances) that it could not be much leſs than a mile, or a mile and half, and I think it is probable, it did not exceed three miles.

CONCLUSION.

105. Thus have I endeavoured to ſhew how the principal phænomena of earthquakes may be produced, by a cauſe with which none, that I have ſeen, appear to me to be incompatible. As I have not knowingly miſrepreſented any fact, ſo neither have I deſignedly omitted any that appeared to affect the main queſtion; but, that I might not unneceſſarily ſwell what had already much exceeded the limits at firſt intended for it, I have omitted,

106. *Firſt*, Thoſe minuter appearances, which almoſt every reader would eaſily account for, from what has been ſaid already, and which did not ſeem to lead to any thing farther: ſuch, for inſtance, are the ſudden ſtopping and guſhing out of fountains, occaſioned by the opening or contracting of fiſſures; the dizzineſs and ſickneſs people feel, from the almoſt imperceptible wave-like motion, *&c.*

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vapour. I don't find, that this phænomenon, which is a common attendant on earthquakes, was obſerved any-where, at the time of the earthquake of the 1ſt of November 1755, except at Amſterdam, where the mercury ſubſided more than an inch. See Hiſt. and Philoſ. of Earthq. p. 309.

107. *Se-*

107. *Secondly*, Thoſe appearances which ſeemed to depend upon particular circumſtances, and of which, therefore, unleſs we had a more exact knowlege of the countries where they happened, it would have been impoſſible to give any account, without having recourſe to uncertain conjectures; of this kind, was the greater agitation of the waters in the lakes of Switzerland, at the time of the earthquake of the 1ſt of November 1755, than during the * earthquake of the 9th of December following, though the houſes upon the borders of them were more violently ſhaken by the latter. And,

108. *Laſtly*, Thoſe appearances, which only ſeem to have an accidental connection with earthquakes, or the cauſes of them; of this kind, are the effects which, in ſome inſtances perhaps, they produce on the weather; the diſtempers which are ſometimes ſaid to ſucceed them; the diſturbance which, we are told, they have ſometimes occaſioned, during the ſhocks, in the direction of the magnetic needle, *&c.* none of which are obſerved to be conſtant attendants on earthquakes, nor do they ſeem materially to affect the ſolution given either one way or other.

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* See Monſieur Bertrand's *Memoires ſur les tremblemens de Terre.*

LVI. *An Extract of a Letter from Abbé* De la Caille, *F. R. S. and Member of the Royal Academy of Sciences at* Paris, *to* Matthew Maty, *M. D. and F. R. S.*

Paris, Feb. 18, 1760.

Read March 6, 1760. I'LL venture to ſend you ſome of my obſervations on the preſent comet, becauſe bad weather may have prevented it from being ſeen in England.

	Equal time.			Longitude.			North latitude.		
	h	′	″	☊ °	′	″	°	′	″
Feb. 8.	9	29	28	☊ 20	17	22	3	26	42
9.	8	48	50	18	49	18	4	46	28
11.	7	22	35	16	5	3	7	14	50
13.	7	47	4	14	43	21	8	24	18
14.	6	41	0	12	20	18	10	30	9

Theſe obſervations, together with another made at Marſeilles, (on the firſt day) at $9^h\ 55'\ 38''$ equal time, when the longitude of the comet was found in ☊ $23°\ 29'\ 46''$, and its north latitude $31'\ 20''$, have enabled me to compute the elements of its orbit. Its motion is direct. The aſcending node is in ☊ $19°\ 42'\ 0''$, and the place of the perihelion in ♈ $26°\ 41'\ 22''$. The inclination of the orbit is $80°\ 51'\ 30''$, and the diſtance of the perihelion $\frac{85248}{100000}$ of the radius of the orbit of the earth. The comet paſſed the perihelion Nov. 25, 1759, at $20^h\ 55'$, mean time, at Paris. Theſe computations will be further improved by the obſervations I ſtill hope to make; but they are ſufficient to find the comet's place in the heavens.

LVII. *Extracts of ſome Letters from Signor* Abbate de Venuti, *F. R. S. to* J. Nixon, *A. M. and F. R. S. relating to ſeveral Antiquities in* Italy.

Dear Sir, Rome, Sept. 27, 1759.

Read March 20, 1760. —SOME days ago a fire accidentally broke out, where the Antonine column lay, near the Curia Innocenziana on Monte Citorio. It burnt the wooden props or ſcaffolding, which ſupported the column, and did ſuch conſiderable damage to the column itſelf, by cauſing its ſurface to ſhell off, that it will be very difficult to ſet it up, if any attempt ſhould be made for that purpoſe hereafter.

A certain Greek epigram, lately dug up at Naples, relating to a boy, has occaſioned ſeveral grammatical altercations among the literati in that part of the world; and theſe again have produced ſome ſmart lampoons and ſatirical writings on each ſide of the queſtion.

Many ſingular ſtatues and relievos, lately diſcovered, have been ſet up in the gardens of Cardinal Albani, without the Porta Salaria. Theſe, if you were among us, would afford a very agreeable entertainment to your curioſity.

LETTER

LETTER II.

Rome, Dec. 31, 1759.

—I have already met with ſome diſſertations of Mr. Swinton, which ſhew his acute judgment, and excellent learning in the oriental languages; and therefore, I do not queſtion his accuracy in treating of the epocha of the Sidonians. Your extract of his eſſay upon that ſubject has induced me to examine ſome coins ſtampt at Sidon, which I think it needleſs to deſcribe, as the world is already in poſſeſſion of their hiſtory. However, I have taken particular notice of one of that kind, of the ſmalleſt ſize, which, I think, has never been publiſhed. It exhibits the trunk of a palm-tree, or rather the *laſeriptium* of Cyrene, with this legend, ΔΩΙΝΥΣ. On the reverſe, there is a caldron, or vaſe, with theſe letters round it, ΦΥΤΙΔΣΧΛΨΥΤΟ. If Mr. Swinton thinks this coin will be of any ſervice to him in his inquiry, I will ſend a more exact copy of it.

I come now (in compliance with your requeſt) to give you ſome inſcriptions lately dug up among the ruins of antient Rome.

The firſt was diſcovered without the Porta Latina, two miles from the city, and runs thus:

LIBERTI. LIBERTAE
PRISCI. AVG. L. GAMIANI
EX. TESTAMENTO. DESCRIPTV. ITA. VT
CAVTVM. ERAT. SVBSCIPSER
LOCVS. MONIMENTI. SIVE. AGER. EST. VIA. LATIN
AD. MILLIARIVM. V. SVPRA. PONTEM. AD
MONIMENTVM. GAMI. CAESARIS. AGATHOCLIAN

DO. LEGO. LIBERTIS. MEIS. VTRIVSQVE. SEXSVS
POSTERISQVE. EORVM. ET. IIS. QVOS. MANV
MITTI. ROGAVI. IBIQVE. TABERNAM. FIERI.
INQVE. EAM. REM. CONSVMI. HS. N̄.
ARBITRATV. AGATHANGELI. COLLIB.

II.

The ſecond is in my poſſeſſion; and is as follows:

D. M.
SOTER. SERVOS. PVBLICVS.
CASTELLAR. AQVAE. ANNIONIS
VETERIS. FECIT. CONGVGI. BENE
MERENTI. ET. L. CALPVRNIVS
FLAVIANVS. MATRI. BENE
MERENTI. SIBI. ET. SVIS
POSTERISQVE. EORVM.

III.

On a Hermes, without a head.

D. M. V. F.
SEX. SAMNIVS
SEX. F. VVLTVR
APER.
DOMO. NEMAVSO
IIIIVIR. IVREDICVND
PONTIFEX. PVBLICORV
SACRIFICIORVM
PRAEFECTVS. VIGILVM
ET. ARMORVM.

IV.

On the baſis of a ſtatue dreſſed in a toga, are the following letters:

EVENTIVS
P. D. S. C.

I have

I have lately bought a relievo, which, in my opinion, is very ſingular. It is of marble, two palms wide, and one high, and repreſents, in a neat taſte, a Faun, with a tail, and wings; which latter circumſtance has never occured to my obſervation before. He ſeems to be dancing, and his dog at his feet is in the ſame attitude. Near him is a tree, to which is tied a very elegant open chariot, (*thenſa*) and beneath it there appears a table, ſuch as are uſed in entertainments, with a goblet upon it, charged with relievo in emboſſed work.

I have likewiſe, a few days ago, met with a cornelian, on which was engraved a man cloathed with a pallium, and ſitting on a chair: before him there appeared a lighted furnace, and upon it a veſſel of glaſs, or earthen ware. The artiſt himſelf held in his hand a pair of iron pincers, with handles, to take off the veſſel from the fire, without burning himſelf. They demanded ten ſequins for it, which I thought too great a price, and therefore did not purchaſe it.

Remarks on the preceding Extracts; in a Letter to Charles Morton, *M. D. Secretary to the Royal Society: By* John Nixon, *A. M. F. R. S.*

Dear Sir,

Read March 27, 1760. I Beg leave, in purſuance of my promiſe, to tranſmit to you ſome obſervations, which tend, in my opinion, to illuſtrate the preceding extracts of the Abbate de Venuti's letters to me.

The

The column, mentioned in Letter I. is of red granite, and was dug up (together with its pedeſtal) by Clement XI. out of the ruins on Monte Citorio, *A. D.* 1705. Innocent XII. intended to have ſet it up; but the great damage it had ſuffered in clearing the rubbiſh from it, hindered that deſign from being effected.

In 1745, Benedict XIV. removed it from the Piazza di Monte Citorio to its preſent ſituation on the ſame hill, and alſo cauſed the pedeſtal, that belonged to it, to be erected where it now ſtands, being the ſpot deſigned for it by his predeceſſors, mentioned above.

The relievi on the ſides of the pedeſtal refer to the apotheoſis of Antoninus Pius; as the inſcription ſhews, that the column was erected to the memory of that emperor by M. Aurelius (ſtiled in this, as in ſome other antient monuments *, Antoninus) and L. Verus, his adopted ſons, and aſſociates in the government.

Letter II. inſcript. 2. l. 2.—SERVOS. PVBLICVS.] *i. e.* One of thoſe officers, who were to atteſt the ſigning of all public inſtruments—*Cenſores extemplo jam atrium libertatis aſcenderunt, et ibi ſignatis tabellis publicis, clauſoque tabulario, et dimiſſis ſervis publicis, negarunt ſe prius quidquam negotii publici geſturos, quam judicium populi de ſe factum eſſet.* Liv. Hiſt. l. xliii. c. 16. A. U. 584. Vid. Marcell. Donat. in loc.

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* Vid. Bianchini apud Vaſi delle Magnif. di Roma, lib. ii. p. 13, 14.

L. 3.—CASTELLAR.] For explaining this title, it is to be obſerved, that the reſervoirs, which received the waters brought by aqueducts into the city of Rome, were called *caſtella*.

Agrippa in ædilitate ſuâ adjectâ virgine aquâ, cæteris corrivatis atque emendatis, lacus ſeptingentos fecit: præterea ſalientes centum quinque; CASTELLA *centum triginta, complura etiam cultu magnifica.* Plin. Hiſt. Nat. l. xxxvi. c. 15.

There are ſtill extant very conſiderable remains of ſome of theſe *caſtella*, *viz.* that of Aqua Claudia at Porta Maggiore; and another of Aqua Marcia, commonly called I Trofei di Mario, near Sto Euſebio in Rome.

From hence it is obvious to collect, that the office of the *caſtellarius* was to ſuperintend the public reſervoirs above-mentioned, to diſtribute the waters in due proportion into the ſeveral quarters of the city, *&c.* Fabretti has preſerved an inſcription, relating to one of theſe officers, *viz.* the *caſtellarius* of the Aqua Marcia. *Vid. Bianchini apud Vaſi delle Magnif. di Roma, lib.* i. *p.* 31.

Ib. L. 3.—AQVAE. ANNIONIS. VETERIS.] This water was brought to Rome *A. U.* 481. It was taken up from the Anio, or Teverone, above Tibur, (Tivoli) about twenty miles from Rome, and entered the city by the ſame aqueduct, which conveyed the Aqua Appia, near the Porta Capena (Sto Sebaſtiano). From thence it paſſed along between M. Aventinus and Cælius, to the end of the Vicus Publicius, near the modern Scola Greca, where it had its reſervoir contiguous to that of the Aqua Appia above-

above-mentioned. *M. Angel. Roſſi Roma Antica, p.* 229. 231.

The epithet *vetus* diſtinguiſhes this water from that of the *Anio novus*, which was brought to Rome from the diſtance of ſixty-two miles, by the emperor Claudius, in the twelfth year of his reign, about *A. U.* 803, as appears from an inſcription in Vaſi, lib. i. p. 32. To which we may add the teſtimony of Suetonius—*(Claudius) Rivum Anienis novi lapideo opere in urbem perduxit, diviſitque in plurimos et ornatiſſimos lacus.* Suet. Claud. c. xx.

Inſcript. 3. l. 7, 8.—PONTIFEX. PVBLICORV. SACRIFICIORVM.] If this title implies the ſame as that of the *Sacerdotes Publici* in Livy, it comprehended the ſeveral orders of Pontifex Maximus, Pontifex, Augur, Decemvir Sacrorum, *&c.* Liv. Hiſt. lib. xxv. c. 2. xxvi. 23. xli. 21, *&c.* But to which of theſe claſſes SAMNIVS, mentioned in this inſcription, belonged, we cannot determine.

Ib. l. 9.—PRAEFECTVS. VIGILVM.] Some writers refer the firſt inſtitution of a night-watch, for the ſafeguard of the city of Rome from fires, to Auguſtus. *Auguſtus adverſus incendia excubias nocturnas, vigileſque commentus eſt.* Suet. Aug. c. xxx. Ἐπειδὴ ἐν τῷ χρόνῳ τούτῳ, *&c. Cùm eo tempore multa eſſent in urbe incendiis vaſtata, libertinos ſeptem locis ad ea curanda inſtituit (Auguſtus).* Dion. Caſſ. l. lv. A. U. 759. But Livy mentions an office ſubſiſting in Rome, in the time of the republic, as early as *A. U.* 449, *viz.* the *Triumviratus nocturnus*, (Liv. Hiſt. lib. ix. c. 46.) which muſt have been the ſame

ſame in effect with that deſcribed above: for it muſt have been undoubtedly the province of thoſe Triumviri to prevent all manner of diſorders and accidents in the night, eſpecially that moſt calamitous one of fires. This concluſion, in general, reſults from the nature of things; as the particular circumſtance relating to fires may be collected from Tacitus. —*Triumviri, quòd ad incendium in ſacrâ viâ ortum tarde venerant, à tribuno plebis die dictâ damnati.* [Tac. apud Godeler. not. in Liv. ſupra.] Now, in order to reconcile theſe accounts, in ſome degree, we may ſay, that, as the inhabitants of Rome were become more numerous and wealthy, and (conſequently) more licentious, in Auguſtus's time, that emperor found it requiſite to new model the night-watch, by making ſeveral additional improvements in it, with regard, probably, to the number and condition of the perſons, of whom it conſiſted; the quality of the officer, who preſided over it; the appointment of the proper places of rendezvous in the moſt commodious parts of the city, *&c.* And, upon account of theſe regulations made by Auguſtus in the night-watch, ſome hiſtorians may have been induced to conſider him as the founder of this police in the city of Rome.

I am, *&c.*

J. Nixon.

LVIII. *A Catalogue of the* Fifty Plants *from* Chelsea Garden, *presented to the* Royal Society *by the worshipful Company of Apothecaries, for the Year* 1759, *pursuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper Præses, by* John Wilmer, *M. D. clariss. Societatis Pharmaceut.* Lond. *Socius, Hort.* Chelsean. *Præfectus & Prælector. Botanic.*

Read March 27, 1760.

1851 ACER foliis trilobis integerrimis. Flor. Leyd. Prod. 459.
Acer trifolium. C. B. P. 431.

1852 Alsine marina foliis portulacæ. Fl. Pruss. P. 12. Ic. 2.
Anthyllis maritima centifolia. C. B. P. 282.
Alsine centifolia, seu Alsine cruciata marina. Ger. Emac. 622.

1853 Aster foliis lanceolatis acutis scabris, caule simplici, floribus umbellatis terminalibus. Dict. Hort.
Aster ramosus annuus Canad. Mor. Hist.

1854 Azalea ramis infra flores foliosis. Flor. Virg.

1855 Brassica foliis infimis ovato-lanceolatis, serrato-dentatis, summis linearibus integerrimis.

1856 Bromos panicula nutante, foliis utrinque subpilosis, glumis ciliatis. Lin. Sp. Pl. 76.

1857 Calendula foliis obverse ovatis denticulatis, caule fruticoso perenni. Flor. Leyd. Prod. 531.

1858 Cerasus foliis ovatis. Virg. Hort. Cliff. 43.
Cerasus

Ceraſus Sylveſtris amara Mahaleb putata. I. B. 1. 227.

1859 Chelidonium maximum Canadenſe acaulon Cornuti.
Sanguinaria. Hort. Eltham.

1860 Clematis compoſitis et decompoſitis foliolis ternatis ſerratis. Gmel.

1861 Clematis purpurea repens, petalis florum coriaceis. Baniſter.

1862 Cratægus; folio ſubrotundo, ſerrato, ſubtus incano, Aria multis. Tourn. 633.

1863 Cratægus; Ceraſi folio, floribus magnis. Amman. Ruth. 274.

1864 Cratægus; folio laciniato. Tourn. 633.
Sorbus torminalis. Dod. 803.

1865 Dens canis latiore; rotundioreque folio, flore ex purpura rubente. C. B. 87.

1866 Elymus ſpica erecta, ſpicula involucro longiore. Hort. Upſal. 22.

1867 Erigeron foliis baſi revolutis. Lin. Sp. Pl. 863.
Senecio Bonarienſis purpuraſcens, foliis imis coronopi. Hort. Eltham. 344.

1868 Eruca latifolia, alba ſativa, Doſcoridis. C. B. 98. Officin. 185.

1869 Euonymus floribus omnibus quinquefidis. Lin. Sp. Pl. 197.
Euonymus Virginianus pyracanthæ foliis, capſula verrucarum inſtar exaſperata. Pluk. Almag. 139.

1870 Gypſophylla foliis lanceolato-linearibus obſolete triquetris lævibus obtuſis. Lin. Gen. nov. 1103.

Saponaria petalis ovatis, foliis glaucis pulpoſis. Hall. Jen. 117.

1871 Hypericum fœtidum fruteſcens majus. Tourn.
Hypericum fruteſcens Canarienſe multiflorum. Hort. Amſt.

1872 Hypericum fœtidum fruteſcens. Tourn.
Tragium legitimum veterum. Ponæ Baldo 88.

1873 Hypericum orientale fœtido ſimile ſed inodorum. T. Cor.

1874 Jaſminum foliis lanceolatis oppoſitis integerrimis calycibus acutioribus. Miller's Icons.

1875 Ixia planifolia, caule multiflora, ſpatha breviſſima. Miller.

1876 Lunaria major; ſiliqua rotundiore. I. B. 2. 881.

1877 Lycopus foliis æqualiter ſerratis. Lin. Sp. Plant. 21.
Lycopus foliis lanceolatis tenuiſſime ſerratis. Flor. Virg. 8.

1878 Majorana rotundifolia ſcutellata exotica. H. R. Par.

1879 Ortega. Læſl. epiſt. viii. Lin. Sp. Pl. 560.
Juncaria Salmatica. Cluſ. Hiſt. 1. P. 174.

1880 Oſteoſpermum foliis oppoſitis palmatis. Lin.
Chryſanthemum perenne Virginianum majus platani folio orientale. Mor. Hiſt.

1881 Oxalis ſcapo umbellifero, foliis ternatis bipartitis. Lin. Sp. Pl. 434.
Oxalis bulboſa pentaphylla et hexaphylla, floribus magnis luteis et copioſis. Burman. Plant. Afric. 80.

1882 Peucedanum Germanicum. C. B. P. 149. Mor. Umb. 36.

Fœniculum

Fœniculum porcinum. Lobel. Adverſ. 350.

1883 Potentilla foliis bipinnatis, ſegmentis integerrimis diſtantibus ſubtus tomentoſis, caule decumbente. Lin. Sp. Pl. 496.
Pentaphylloides ſupinum minus, folio glabro non ſerrato. Amman. Ruth. 116.

1884 Robinia pedunculis ſimpliciſſimis, foliis abrupte pinnatis. Hort. Upſal. 212.
Caragana Siberica. Flor. Leyd. 537.

1885 Salvia foliis oblongo-ovatis integerrimis calycibus patulis coloratis. Fig. Tab. 225. F. 2.

1886 Scabioſa Africana fruteſcens, foliis rugoſis, et crenatis minor. Par. Bat.

1887 Seſeli caule alto rigido, foliolis linearibus faſciculatis. Lin. Sp. Pl. 260.
Oenanthe ſtriata rigida. Hort. Cliff. 99.

1888 Silene floribus faſciculatis, calycibus tomentoſis. Fl. Virg.

1889 Silene foliis quaternis. Flor. Virg.
Lychnis Caryophylleus Virginianus, Gentianæ foliis glabris quatuor ex ſingulis geniculis caulem amplexantibus, flore amplo fimbriato. Raii Hiſt. tom. ii. p. 1895.

1890 Siler montanum, foliis Aquilegiæ. Mor. Hiſt. 3. 276.
Angelica montana perennis, Aquilegiæ folio. Tourn. 313.

1891 Siſyrinchium foliis gladiolatis amplexicaulibus pedunculis brevioribus. Dict. Hort.
Bermudiana iridis folio fibroſa radice. T. Inſt. 338.

1892 Soli-

1892 Solidago paniculata, racemis recurvatis, floribus erectis, foliis lanceolatis integerrimis glabris. Miller's Icons.

1893 Sorbus Sylveſtris foliis domeſticæ ſimilis. C. B. P. 415.
Sylveſtris ſive Fraxinus bubula. Ger. 1290.

1894 Styrax; folio Mali Cotonei. C. B. 452. Officin. 473.

1895 Thapſia montana omnium maxima, foliis lobatis, umbellis luteis. Micheli Hort. Flor.

1896 Tithymalus arboreus. Alpini.

1897 Tithymalus Cypariſſias. Alpini Exotic.

1898 Veronica; floſculis oblongis pediculis inſidentibus, Chamædryos folio. Hiſt. Ox. 2. 322.

1899 Viburnum Mathioli. C. B. 429.

1900 Vitex foliis ternatis quinatiſque pinnato inciſis, ſpicis verticillatis terminalibus. Dict. Hort.

LIX. *Some Account of the Animal ſent from the* Eaſt Indies, *by General* Clive, *to his Royal Highneſs the Duke of* Cumberland, *which is now in the Tower of* London: *In a Letter from* James Parſons, *M. D. F. R. S. to the Rev.* Tho. Birch, *D. D. Secretary to the Royal Society.*

Read March 27, 1760.

AT the requeſt of the Reverend Dr. Littleton, dean of Exeter, I went to obſerve this creature, in order to find what claſs of animals he belonged to; and made the drawing,



I Parsons MD. *ad vivum del. mart 25. 1760.*

J. Mynde. Sc.

ing, now before the Royal Society, for its inſpection. [See *Tab.* XIV.] I have endeavoured to make it as accurate as poſſible in all its proportion; yet am afraid I have made the ears a ſmall matter too long. There is a figure of it in the London Magazine for December laſt, which has no reſemblance at all to it, except in the ears, which the engraver, who drew it, has made to turn forwards, contrary to nature. However, the following deſcription will, I believe, be the proper account of it.

It is ſomething taller than the largeſt ſized cat, being about fifteen inches high at the ſhoulders; ſlender and light, though ſtrong. The head is ſmall in proportion to the reſt, and the neck ſlender. It has nothing fierce in its aſpect, but is mild, and very tame. It is exactly of a fawn-colour, having its ears black on their outſides, and lined with white hairs, and ſome white round the root of each ear; it is alſo white under the throat and belly, and a little ſo on the backs of its limbs. Its eyes are ſmall, and its head like that of a cat, but ſomewhat ſlenderer; its legs are genteel and ſtrait, with the paws of a cat, having the power of dilating and contracting its toes, which are armed with ſtrong crooked nails, in the ſame manner as a cat or tyger does; and its actions are like thoſe of a cat. I ſat and watched its motions, and ſaw it lick its foot, and rub it over its face ſeveral times, exactly like a cat; and was told by the man who ſhewed it me, that, if it is offended, it hiſſes. I examined its teeth, and find them in the ſame number and manner with thoſe of a cat. And as to its food, they give it raw mutton every day; and when it is ſick, which it often is, they give it a live

XIV. *p.649*

J. Mynde. Sc.

live fowl, or rabbit, which it ſeizes eagerly, and lies upon it without motion, for a conſiderable time, to ſuck the blood, and this proves a certain cure. The figure ſhews it to have alſo a tail like that of a cat.

None of the natural hiſtorians have any account of this animal, that I have yet ſeen, except the learned Dr. Walter Charleton *, who has a bad figure of it, engraved at the expence of Dr. John Lawſon, his cotemporary, of the college of phyſicians, as it appears in an inſcription at the bottom of the plate, wherein the head is, contrary to truth, very large and ſtrong in appearance, the tail like that of a fox, and the whole as ſtrong as a maſtiff dog: the name given it in the plate is the ſame with this, but differently ſpelled, thus, *Siyah-ghuſh*.

This author very juſtly ranks it among the cats, and has given ſuch an account of this animal, as well deſerves the notice of this learned Society, of which I have made the following Engliſh extract.

" Among the wild cats, which vary according to " the difference of climate, manner of living, and " the like, none is more worthy of notice than that " which is now kept in the park of our ſovereign " King Charles II. It was ſent to the king by an " Engliſh gentleman, who was governor of our mer- " cantile affairs in the dominion of Surat, and is called, " among other names, in the Perſian language, *Siyah-*

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* See his Exercitationes de Differentiis et Nominibus Animalium.

" *Ghuſh*,

" *ghuſh*, that is, *Black-ear* *, all along the coaſt of " Coromandel, and indeed all over India. It is about " the ſize of a fox, but like a cat in its form; and " has the cunning and cruelty of a leopard, with " the limbs of a cat, but longer and ſtronger; having " ſo much ſtrength, that I ſaw it kill a hound, that " came in its way, in a moment. The legs are " thick ſet with hair, and its nails concealed under " them, which are never extended but upon ſeizing " its prey, which is common to lions, panthers, and " domeſtic cats. But what ſeems peculiar to this " animal is, that, having jumped upon his prey, he " lies upon it unmoved, holding his bite, as if he " was dead, whether for joy, or in order to drink the " blood of the creature. The great men in India " have them bred up tame, becauſe of their dex- " terity in catching birds, hares, rabbits, and ſuch- " like; and ſuch is their craft and fierceneſs, that " they will ſeize even a fox; but their keepers will " not ſuffer them to attack any thing above their " ſtrength, and therefore they only ſet them at cranes, " geeſe, ducks, pheaſants, partridges, peacocks, and " ſuch-like game, which they ſeize by many kinds " of deceit, to the great pleaſure of the ſpectators; " and catch thoſe timid animals, the hare, rabbit, fawn, " goat, *&c.* by ſwift running, and ſometimes by " craft.

" When they are ſick (which, from over-gorging " their ſtomachs, they often are) their keepers ſteep

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* Dr. Charleton ſays, that he was obliged to his good friend the learned Dr. *Thomas Hyde*, then principal librarian to the Bodleian library, for this interpretation, who was well verſed in the oriental languages.

" a piece of tender meat in human urine, and feed " them with it, and being bruiſed or tired by over- " hunting, they give them ſome *Mummy*, wrapped up " in their meat, and a warm place to reſt in till they " recover."

It is ſaid of this animal, that he follows the lion at a diſtance, in order to feed on what he leaves of the animals he deſtroys. To illuſtrate this, Dr. Charleton quotes a paſſage from the *Apolog.* of *Sheich Saadi*, which was written five hundred years before, and publiſhed in Perſic and Latin by *Georgius Gentius*; which is in Engliſh as follows:

—This *Black-Ear* is aſked, " What makes him " keep company with the lion, and ſeems ſo officious " to pleaſe him?" to which he anſwers, " That I " may feed on his leavings, and lead my life ſafely " under his protection." To which it is replied, " Since you live under the ſhadow of his protection, " and draw ſuch benefit from him, why do not you " approach nearer to him?" He anſwers, " If I took " your counſel, and come near to him, I ſhould not " be ſafe from his fury a moment."

Now from this particular account by the learned Charleton, and my own obſervations on him and his actions, I am inclined to rank this animal among the cats; and join with *Linnæus*, who, in his *Ordo ſecund.* has a fifth ſpecies of *Felis*, which agrees well with the principal characters of the animal before us: his words are,

" *Felis cauda elongata, auribus——*
" *——————penicilliformibus.*"

LX. *An Account of the Frog-fiſh of* Surinam, *addreſſed to the Royal Society, by Mr.* George Edwards, *F. R. S.*

Read March 27, 1760.

I Have now the honour to lay before this Royal Society an animal not to be found in the Britiſh Muſeum, nor in any other collection that I have ſeen in England, and which, perhaps, deſerves attention, in regard to what is ſaid of its ſtrange metamorphoſes, as much as any part of natural hiſtory whatever. It was brought from Surinam in South America, by the way of Barbadoes, to John Fothergill, M. D. of London, and is the animal, which Merian and Seba deſcribe as changing from a frog into a fiſh.

In the appendix to Merian's Nat. Hiſtory of the Inſects of Surinam, where ſhe treats of thetrans formation of fiſhes into frogs, and of frogs into fiſhes, after explaining, how the European frog is changed from a minute fiſh (or tadpole *) into a perfect frog, ſhe proceeds to deſcribe the gradual transformation of a ſpecies of frogs found in great numbers in the river of Surinam, into perfect fiſhes, and gives five figures to illuſtrate her deſcription; the ſubjects whereof, ſhe ſays, were then in the collection of Albert Seba at Amſterdam, from whom ſhe alſo had her

* I have groſsly copied Mrs. Merian's five figures from Plate LXXI, the better to explain her deſcriptions, which figures are herewith preſented. Linnæus calls this animal Paradoxa, Laſt Edit. of his Syſtema Naturæ, p. 212.

figures and information, as appears ſince by the account publiſhed by Mr. Seba of his curious cabinet of natural hiſtory, in two pompous folio volumes, a copy of which, finely illuminated, is now in the Britiſh Muſeum.

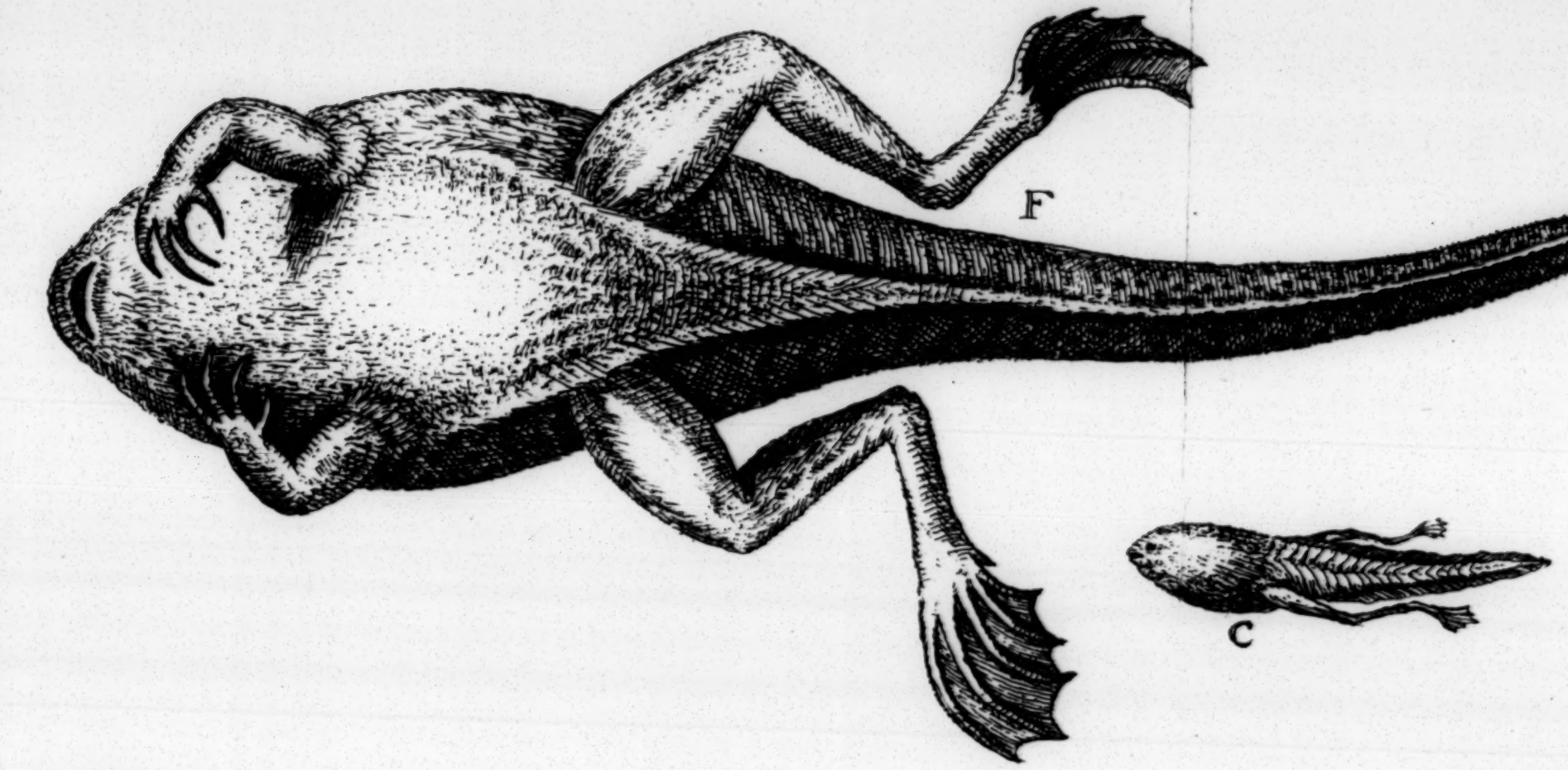
Mrs. Merian's Deſcription is as follows.

" The firſt figure ſhews the perfect frog, brown, " yellow, and green, in ſpots, but paler on the belly, " the hinder feet webbed like thoſe of a gooſe, the " fore feet without webs: the ſecond figure repre- " ſents the firſt change the frog undergoes, by the " growing out of a tail; after which it gradually ac- " quires the ſhape of a fiſh, the two fore feet decreaſ- " ing and periſhing by degrees, as is ſhewn by figure " 3. alſo the decreaſe of the hinder legs, as is " ſhewn by figure 4. and, laſtly, the frog is changed " to a perfect fiſh, as at figure 5.

" The Indians and Europeans of Surinam call " theſe fiſhes Jakies; they are cartilaginous, of " a ſubſtance like our Muſtela †, and very deli- " cate food. A bone (or cartilage) runs down the " back, with ſmall bones all over the body, which " is divided into equal parts; they are adorned with " tender beautiful ſcales, are firſt of a darkiſh co- " lour, and afterwards grey."

The frogs of Aſia and Africa are deſcribed by this author, plate 72. to change gradually from fiſhes into frogs, as the European frogs do; but then ſhe tells

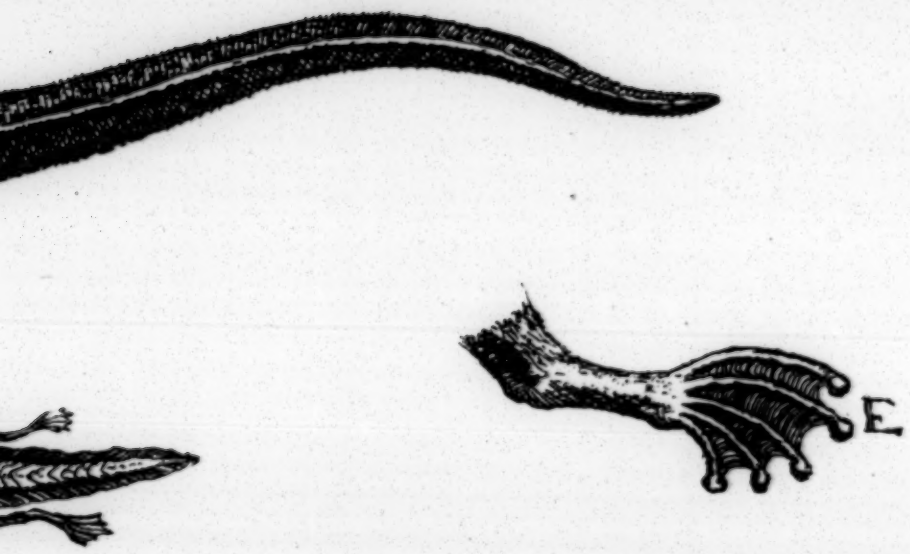
† A fiſh of the cartilaginous kind: See Willoughby de Piſcibus, Tab. B. 5. and Edwards's Gleanings, Tab. 288.







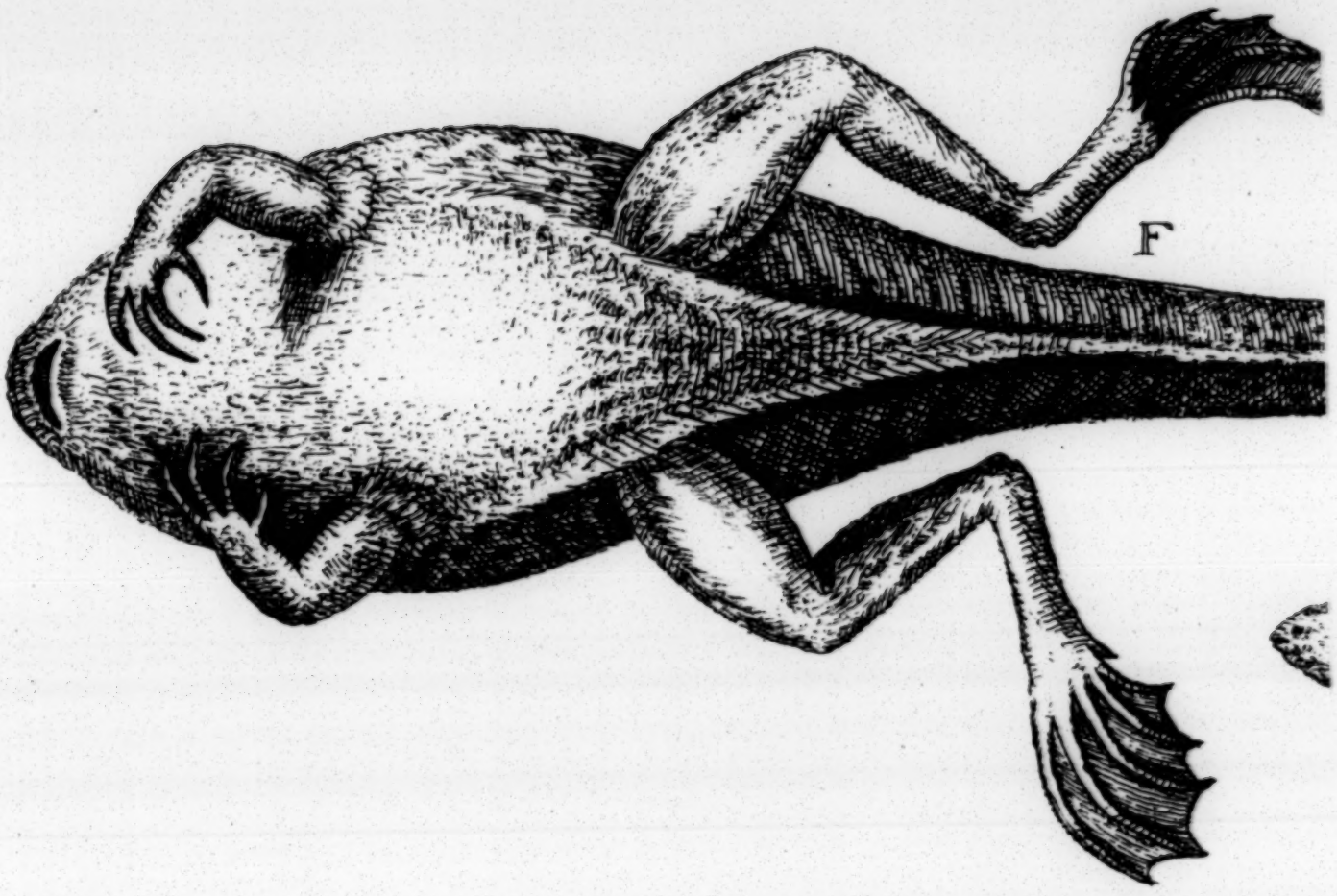
The Frogfish of Surinam Drawn from nature of the size of Life. George Edwar





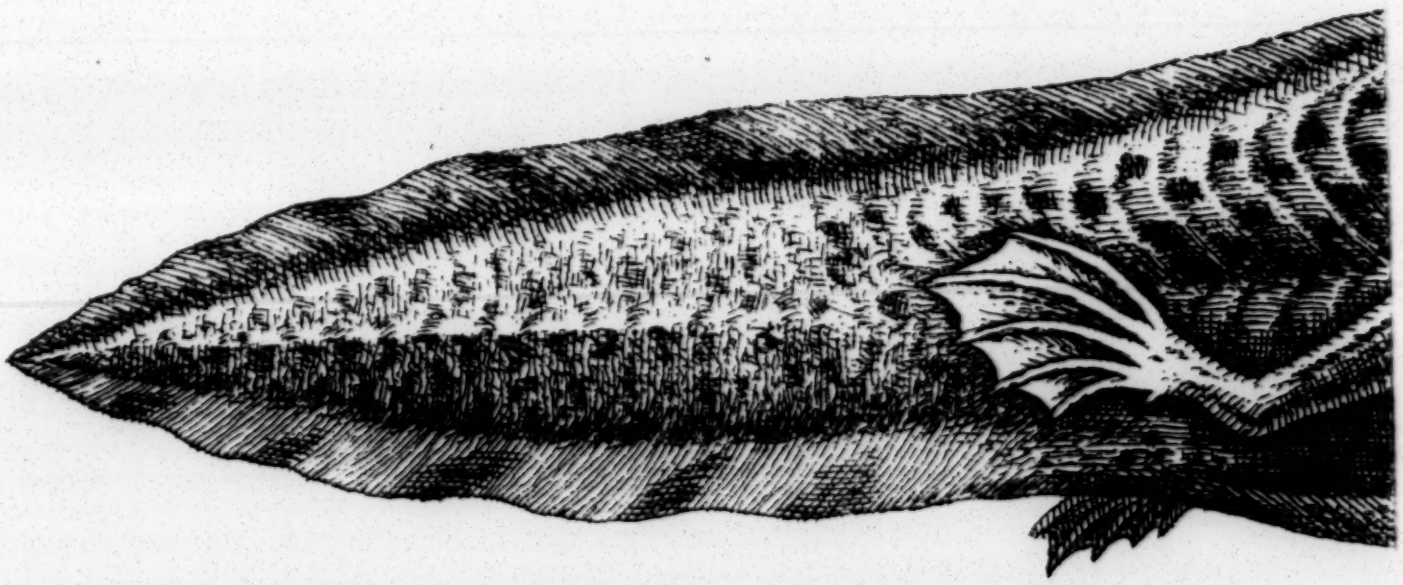


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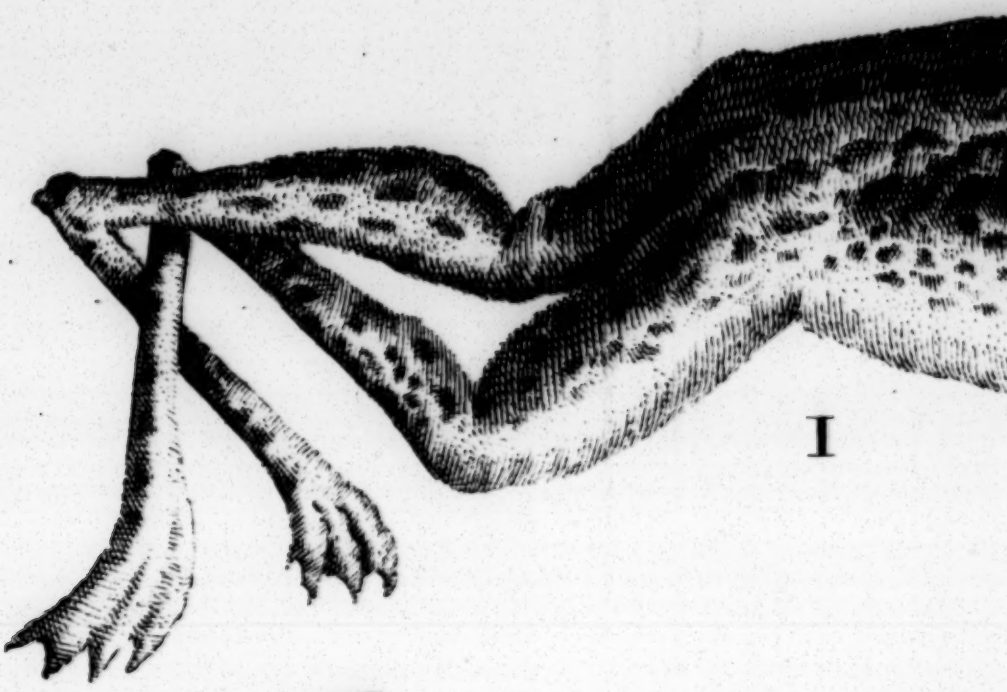






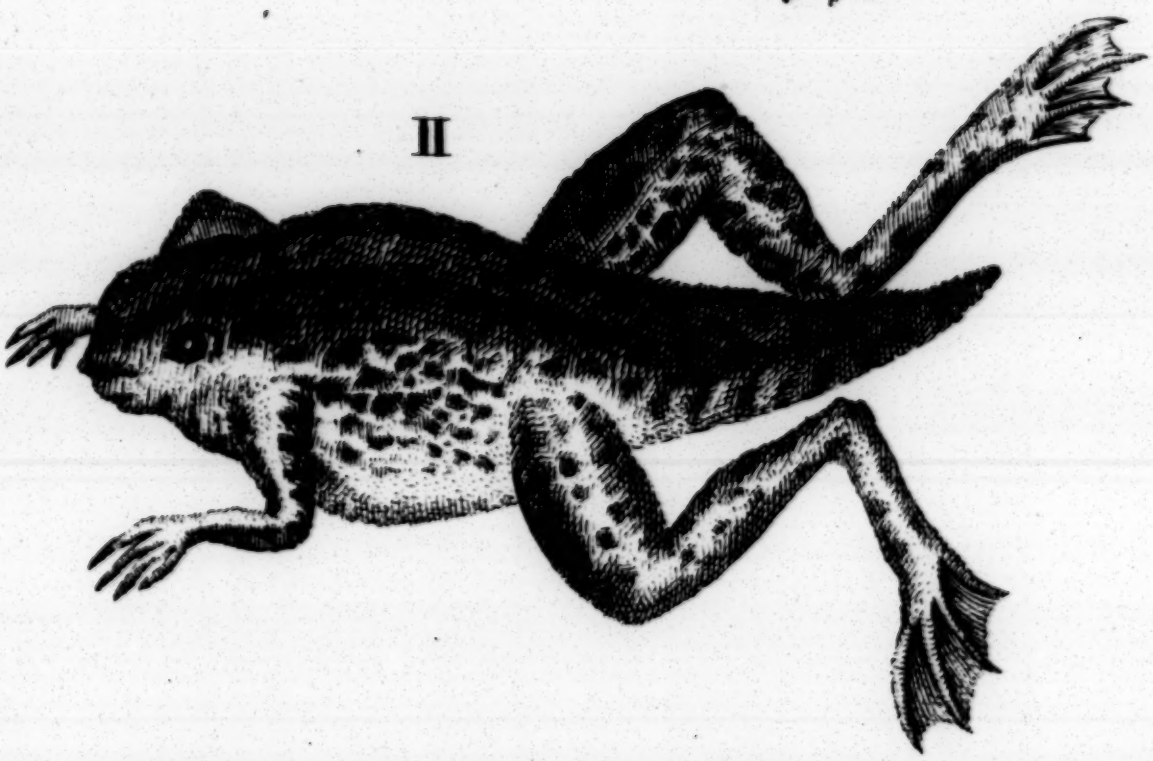


The Frogfish of Surinam Drawn from nature of the size of Life.



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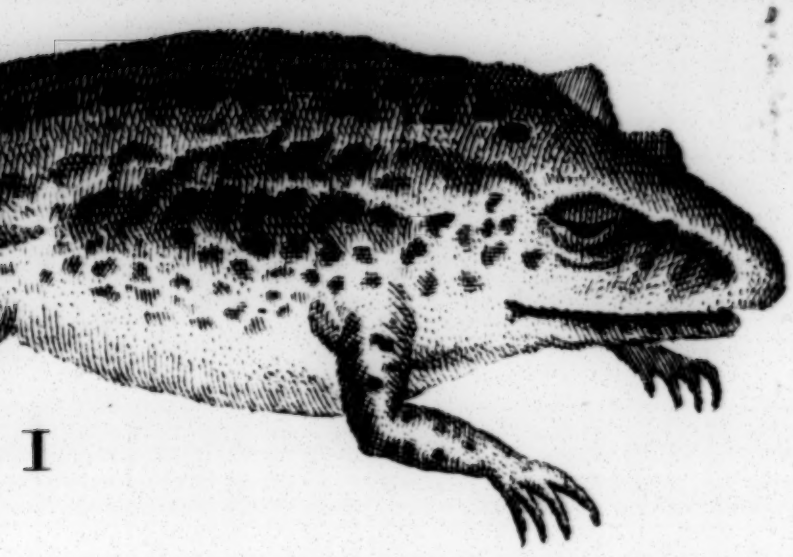


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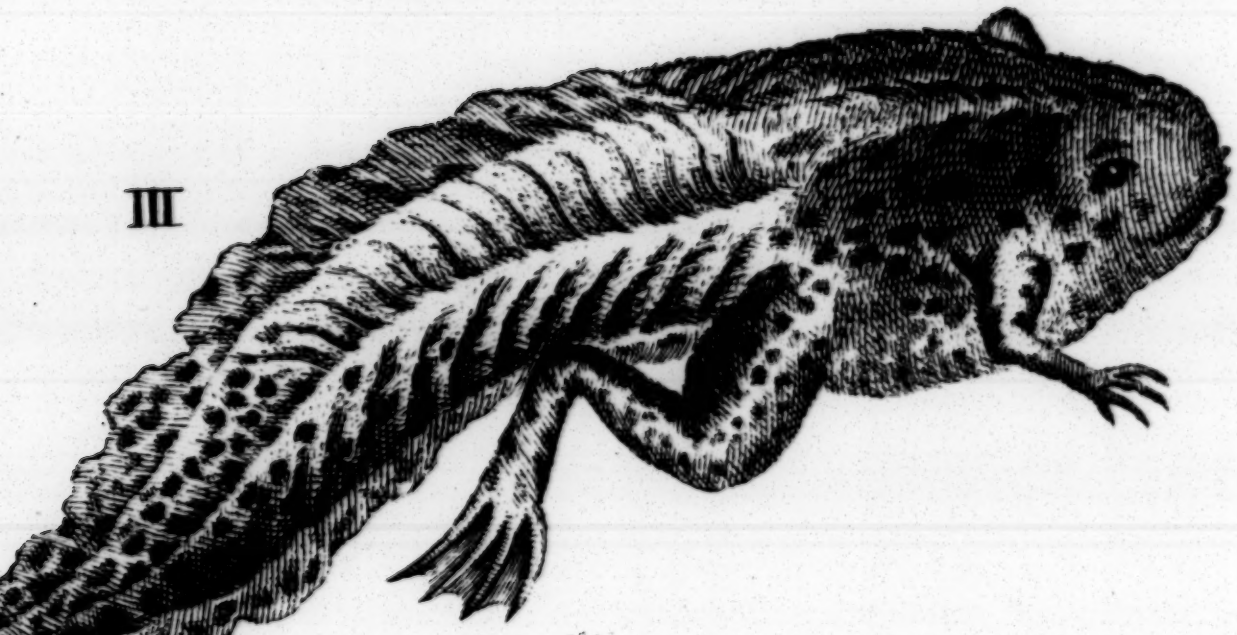


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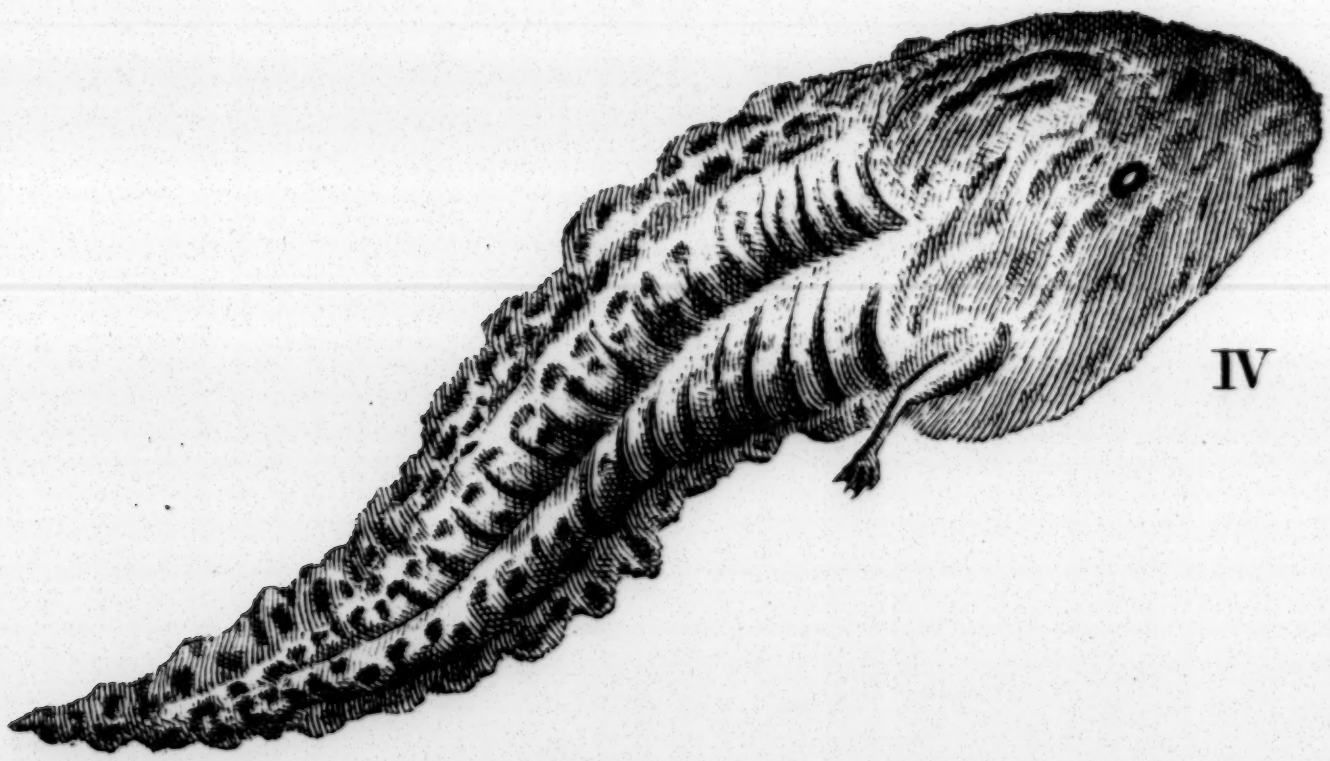




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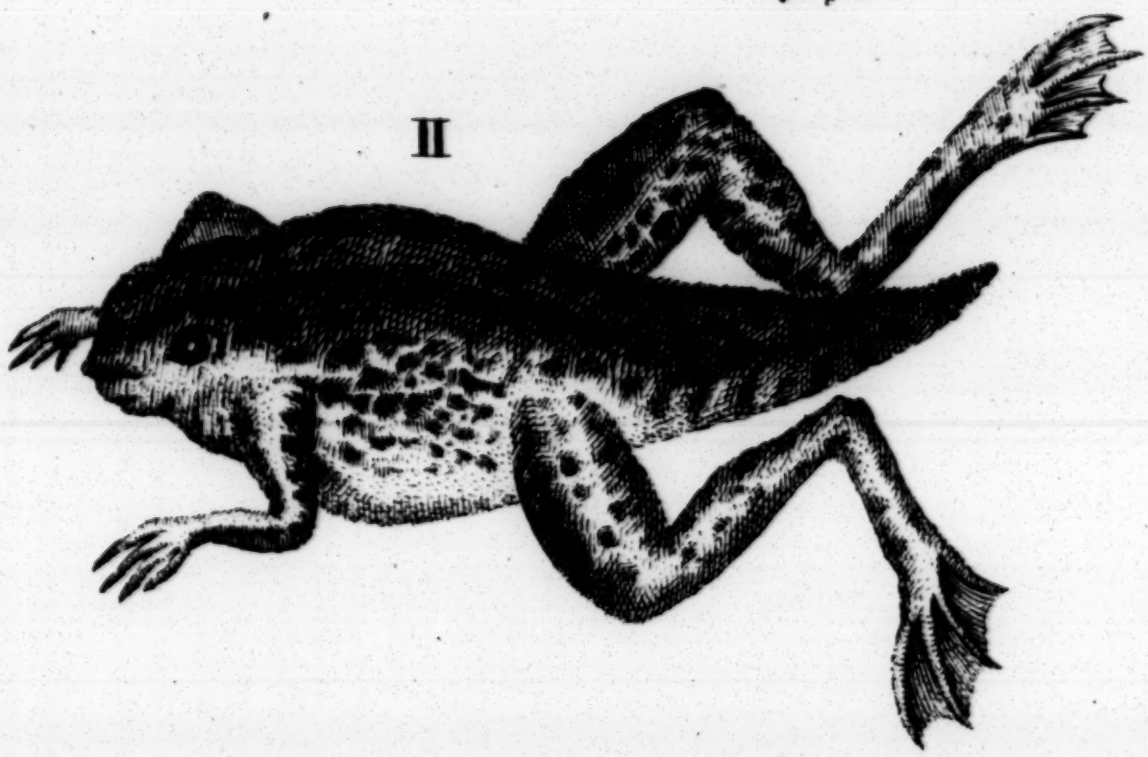


IV

J. Mynde sc.



II



V



us that, after many years growth, they change again into fiſhes, tho' the manner of their change is unknown.

The natural ſubject before us, two views of which I have given in a print annexed, ſee Tab. XV. Letters F G. They are drawn and engraved immediately from nature, of the ſize of life, which, on compariſon, I find to be more than double the ſize of what Merian has figured in her book, plate 71. which inclines me to think this before us to be ſpecifically different from Merian's: her figure expreſſes the fin, which paſſes round the tail, ſcalloped, whereas in this before us the edge of the fin is perfectly even, without any indentures: the hinder feet in her figures have only four toes each, whereas this before us hath five, webbed together, beſides a ſmall rudiment of a toe, as may be ſeen in the natural ſubject, as well as in my print. The fore feet, both in Merian's and my figures, expreſs four toes, unwebbed, tho' I find the four toes to ſtand, three of them in the form of fingers, and one oppoſite to them, which ſerves as a thumb, which is not expreſſed in Merian's figure. The general ſhape of it is expreſſed by the figures F. G, in my print. The ſkin is ſoft, without any thing of ſcaline ſs, and the fin round the tail is like thin wet leather, without any fibres to ſupport it, as is common to moſt fiſhes. The toes in all the feet end in points, but are without nails or claws. On piercing the tail, or hinder part, with a needle, I found no bony reſiſtance; but on piercing the thighs and head the bones ſtrongly reſiſted. Its colour in the ſpirits appeared dark brown, with duſky ſpots, the under ſide lighter, and without ſpots.

There

There came in the ſame glaſs of ſpirits with the above-deſcribed frog-fiſh (as I have choſe to call it), a parcel of ſmall tadpoles, in their different changes from the ſpawn to near the perfect frog: ſee them expreſſed on the plate of their natural ſize at letter A B C D. Doubtleſs theſe produce a ſpecies of frogs different from the European, and perhaps are the ſame that Merian and Seba deſcribe as changing into fiſhes. I think, however, that our aſſent to ſuch an opinion may reaſonably be ſuſpended, till we are confirmed in it by farther obſervations of the real fact; for it ſeems ſtrange, that a tadpole ſhould firſt be changed into a frog, and that the ſelf-ſame frog, by a reverſed proceſs of nature, ſhould change again into a very large tadpole, as figured at G in the plate, and finally change into a fiſh, as in Merian, tab. 71. fig. 5. It ſeems very ſtrange, that another tail ſhould grow from the frog that hath lately loſt one, and that he ſhould gradually loſe his legs, and become a perfect fiſh. Nature, in her ordinary courſe, is not accuſtomed to act in ſuch a manner backwards and forwards, to ſeem to perfect a work, and then to reverſe it by a proceſs directly oppoſite.

The little tadpoles on the plate A B C D, are ſpecifically different from the large one F G, as is manifeſt by the difference of their feet: ſee the hinder foot of one of the ſmall ones magnified at E, which ſhews, that the ends of their toes are round and flat on their underſides, both in the hinder and fore feet: forwards they have four toes in each foot, unwebbed; the hinder feet have five toes, each webbed together.

As to the ſpecimen before us, whether it is already in its perfect ſtate, a ſpecies of frog having a tail, or a kind

kind of water-lizard, I ſhall not pretend to determine: but, conſidering its ſize, if it ſhould be deemed a tadpole, as firſt produced from a ſpawn, and in its progreſs towards a frog, ſuch a frog, when full-grown, if it bears the ſame proportion to its tadpole that the frogs in Europe do, it muſt be of an enormous ſize; for our full-grown frogs exceed the tadpoles, and young fry of perfect frogs, at leaſt fifty times in magnitude.

London, March 27, 1760.

LXI. *An Account of a remarkable Operation on a broken Arm; by Mr.* Charles White, *Surgeon at* Mancheſter, *communicated by* George Lloyd, *Eſq; F. R. S.*

Read March 27, 1760.

ROBERT Elliot, of Eyam, in Derbyſhire, a very healthful boy of nine years old, had the misfortune, about midſummer in the year 1759, by a fall, to fracture the *Humerus*, near the middle of the bone. He was immediately taken to a bone-ſetter in that neighbourhood, who applied a bandage and ſplints to his arm, and treated him as properly, as, I ſuppoſe, he was capable of, for two or three months. His endeavours, however, were by no means productive of the deſired effect, the bones not being at all united. A ſurgeon of eminence in Bakewell was afterwards called in; but as he ſoon found he could be of no ſervice to him, and, as the caſe was very curious, he adviſed the lad's friends

friends to ſend him to the infirmary at Mancheſter: he was accordingly brought thither the Chriſtmas following, and admitted an in-patient. Upon examination, we found it to have been a ſimple oblique fracture, and that the ends of the bone rode over each other. His arm was become not only entirely uſeleſs, but even a burthen to him, and not likely to be otherwiſe, as there was little probability, that it would ever unite, it being now near ſix months ſince the accident happened.

Amputation was therefore propoſed as the only method of relief; but I could not give my conſent to that; for, as the boy was young, and had a good conſtitution, it was hardly poſſible that it could be owing to any fault in the ſolids or fluids, but that either nature was diſappointed in her work by frequent friction, while the callus was forming, or rather that the oblique ends of the bone being ſharp, had divided a part of a muſcle, and ſome portion of it had probably inſinuated itſelf betwixt the two ends of the bone, preventing their union. Which ever of theſe might be the caſe, I was of opinion, that he might be relieved by the following operation; *viz.* to make a longitudinal inciſion down to the bone, to bring out one of the ends of it (which might be done with great eaſe, as the arm was very flexible), and cut off the oblique end, either by the ſaw or cutting pincers, then to bring out the other end of the bone, and cut off that likewiſe; afterwards to replace them end to end, and then treat it intirely as a compound fracture.

The objections, made by the other gentlemen concerned to this propoſal were, 1ſt, The danger of wounding

wounding the humeral artery by the knife; 2d, the laceration of the artery by bringing out the ends of the bones; and, 3d, that we had no authority for ſuch an operation. As to the firſt, that was eaſily obviated, by making the inciſion on the ſide of the arm oppoſite to the humeral artery. The place of election appeared to me, to be at the external and lower edge of the deltoid muſcle, as the fracture was very near to the inſertion of that muſcle into the humerus, the danger of wounding the veſſel not only being by that means avoided, but after the operation, while the patient was confined to his bed, the matter would be prevented from lodging, and the wound be eaſily come at, to renew the dreſſings. The ſecond objection will not appear to be very great, when we conſider, that in compound fractures, the bone is frequently thruſt with great violence through the integuments, and ſeldom attended with the laceration of any conſiderable artery, and as this would be done with great care and caution, that danger would appear very trifling. The third and laſt Objection is no more than a general one to all improvements.

This method, which I have been propoſing, was at laſt reſolved upon, and I aſſiſted in the operation, which was performed by a gentleman of great abilities in his profeſſion, on the third of January, in the preſent year. The patient did not loſe above a ſpoonful of blood in the operation, though the tourniquet was not made uſe of. When the operation and dreſſings were finiſhed, the limb was placed in a fracture-box contrived on purpoſe, the lad confined to his bed, and the reſt of the treatment nothing different from that of a compound fracture.

The wound was nearly healed in a fortnight's time, when an erifipelas came on, and ſpread itſelf all over the arm, attended with ſome degree of ſwelling: this by fomentations and the antiphlogiſtic method ſoon went off, and the cure proceeded happily without any other interruption. In about ſix weeks after the operation, the callus began to form, and is now grown quite firm: that arm is as long as the other, but ſomewhat ſmaller by ſuch long continued bandage; he daily acquires ſtrength in it, and will ſoon be fit to be diſcharged.

C. White,
Surgeon to the Infirmary at Mancheſter.

Mancheſter,
March 17, 1760.

LXII. *An Account of a Bone found in the Pelvis of a Man at* Bruſſels; *by* Terence Brady, *M. D. In a Letter to the Rev.* Tho. Birch, *D. D. Secretary to the Royal Society.*

Rev^{d}. Sir,

Read Apr. 29, 1760.

I Send you here incloſed a draught of a bone found in the pelvis or baſon of a man, that died in the military hoſpital of this town the 12th of March 1760, of a ſeven days inflammatory diſtemper. This extraordinary concretion weighs about 20 ounces, has all the external appearances of a bone, with the hardneſs, ſolidity, and ſpecifick gravity



J. Mynde sc.

vity of common ſtone. It is chequered or marbled, as you ſee in the figure, *Tab.* XVI. by which you can diſtinguiſh the primitive particles of the bone whiter and harder than the darker part: it was formed on the lower extremity of the meſocolon, and probably as it grew big was carried down by its own weight into the baſon, where it had no adherence to, nor connection with, any of the adjacent parts, but lay in its own very thin membrane or perioſtium, betwixt the os pubis and the bladder, ſomewhat to the right ſide. It was joined to the meſenterium by a tough, compact, membranaceous, glandulous ſubſtance, in the form of an inverted cone, whoſe point was firmly inſerted in the cavity obſerved on the top: here the membranaceous fibres were turned into bones, or, *vice verſa*, the fibres of bones degenerated into fleſhy membranes: there was no intermediate cartilaginous ſubſtance to be obſerved. By drawing up this conic body with very little effort, the bone followed, to the great ſurpriſe of all the ſpectators; after which there was no farther inquiry made, in regard of the other viſcera of the abdomen. It was only taken notice of, that the omentum was quite conſumed, and the meſenterium very much ſwelled and ſchirrous.

The man, that bred this monſtrous bone, was called Straſzet, was born in Bavaria, and lived to the age of 55, of which he was 28 years a muſketeer in Bareith's imperial regiment. He was always ſtrong and healthy till about five years ago, when he began to complain of the hardneſs of his belly, and now and then of a ſuppreſſion of urine, of which laſt inconveniency he could help himſelf, by turning on his right ſide, and lying a little on his face: by that po-

ſition, the ſtone did no more preſs on his bladder. He never miſſed doing ſoldier's duty till his laſt ſickneſs, about ſeven days before his death.

We have examples of membranes, and of ſeveral ſoft parts of the body, being oſſified; but I believe there is not ſuch a monſtrous production as this to be ſee nany-where.

About 20 years ago I ſaw at Mantua, two inches of the aorta near the heart turned to bone, in a man that was a long time tormented with a violent palpitation of heart.

I know, Sir, relations of ſuch extraordinary appearances are often, and with reaſon, ſuſpected of exaggeration, but you may depend upon the veracity of this. I am,

Reverend Sir,

Your moſt humble, and
moſt obedient Servant.

Bruſſels, March 29, 1760.

Terence Brady, *M. D.*

LXIII. *An Account of an extraordinary Caſe of a Lady, who ſwallowed Euphorbium. By Dr.* Willis, *of* Lincoln: *Communicated by* Sir Francis Daſhwood, *Bart.*

Read Apr. 24, 1760.

IN December 1758, Mrs. Willis of Lincoln fell into a ſlow fever, occaſioned by too ſmall a diſcharge of the lochia after lying-in,

lying-in, and a redundancy of milk, the conſequence of her not ſuckling her child. On the 18th day after her delivery, by the miſtake of her nurſe, ſhe took, inſtead of a draught that was ordered for her, two ounces of the tincture of euphorbium *. The ſhocking ſymptoms, which immediately enſued, violent ſuffocation, and an intolerable burning pain in the mouth, throat, and ſtomach, ſoon diſcovered the horrible miſtake. I was in the room in about four or five minutes after the accident happened, unappriſed of the nature of it, and therefore the more ſhocked, when I found every body in tears of deſpair, offering at no means of relief, as they had no hopes of ſucceſs.

As ſoon as I was made acquainted with what had happened, it occurred to me, that warm water and oil were the likelieſt things to correct and expel the poiſon. I imagined a large quantity of warm water might probably make the patient vomit, and in ſome meaſure help to diſcharge the cauſtic tincture. I was ſure the water would at the ſame time mitigate its violence, by diluting it, and by precipitating the acrid gum from the ſpirit, whereby it would neceſſarily be hindered from touching the membranes of the ſtomach and bowels in ſo many points, and from penetrating into their ſubſtance.

There was happily a large tea-kettle of water on the fire, of which, being firſt qualified with a proper

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* The tincture was thus made;

℞ *Gum. Euphorb.* ℥ij. *Spt. Vin. rectif.* ℥ij. *Sol. add. Camph.* ℥ij. The camphor was ordered to weaken the cauſtic quality of the tincture, which being applied to a horſe's leg without the camphor had made a blemiſh.

quantity

quantity of cold water, I immediately gave the patient a baſin lukewarm, and repeated it as faſt as poſſible, conjuring her to uſe her utmoſt reſolution to ſwallow; which ſhe certainly did in a moſt ſurpriſing manner. After the third baſin, ſhe vomited very freely: what was brought up ſmelt very ſtrong of the camphor, and ſeemed to contain a good deal of the tincture, with the gum ſeparated from the ſpirit. She ſtill drank on, but complained of exceſſive burning and torture in her ſtomach, crying out continually, ſhe was burnt to death.

I had then recourſe to oil between whiles, in the quantity of two or three ounces at a time; and drenched her plentifully ſometimes with oil and ſometimes with water. She vomited very copiouſly, and I repeated the oil and water interchangeably, till ſhe had taken, as well as I could gueſs by the veſſels, two gallons of water and a flaſk of oil in a very ſhort time †.

Imagining

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† Dr. Sydenham, being called to a man, who had taken *Mercur. ſublim. corroſ.* about an hour before the doctor ſaw him, the poiſon having affected his lips, &c. only ordered water to be taken in a large quantity, and thrown up copiouſly in gliſters. But as the corroſive ſublimate of mercury is to be conſidered as a poiſon, whoſe cauſtic acrimony conſiſts in a ſaline principle, and water is the proper ſolvent, diluent, and vehicle of all ſaline ſubſtances, the propriety of Sydenham's ordering water alone is ſufficiently apparent. Poiſons of a ſaline nature being diſſolved in the fluids of the ſtomach and inteſtines, do not confine their ravages to theſe parts only, but are apt to enter the abſorbent veſſels, and inſinuate themſelves into the road of the circulation. Water is here a good antidote, as it dilutes ſuch ſubſtances, waſhes them off the ſenſible membranes, deſtroys their acrimony, and readily paſſing through all ſorts of canals, ſoon

Imagining the deleterious draught had not had ſufficient time to bring on any violent inflammation or excoriation, or to make its way into the blood, vomiting and purging, with plenty of diluents and ſheathing ſubſtances, ſeemed the likelieſt means to ſave the patient, if any thing could be hoped to ſucceed in ſo perilous a ſituation. I therefore ordered a mild but operative emetic of *Pulv. Rad. Ipecacuan.* and a mixture with *Sperm. Ceti* and oil to be taken occaſionally; ſtill following up the patient with oil and water. I had reaſon to expect the emetic would alſo purge as well as vomit, and not only clear the ſtomach of the remains of the poiſonous draught, but likewiſe carry downwards what portion of it might have paſſed through the pylorus by the contraction and agitation of the ventricle on the preceding vomiting. The apothecary demurred at the emetic, and

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ſoon carries them out of the body. But the caſe is otherwiſe with gummy reſinous poiſons, ſuch as euphorbium. Theſe being indiſſoluble in water, are not ſo apt to enter the abſorbent veſſels, and paſs into the blood, but, by their acrimony, ſhut up the orifices of thoſe canals, and preclude a paſſage. Therefore oil here ſhould be called in to the aſſiſtance of water. For the cauſtic reſinous ſubſtance of euphorbium being precipitated or ſeparated from the ſpirit, and formed into clots by the water, would ſtill be apt to ſtick to the tender nervous membranes of the ſtomach and bowels, and by its intolerable acrimony cauſe violent vellications, inflammations, and gangrene. But the oil contributes greatly to prevent theſe fatal effects, by ſheathing the corroſive acrimony of the poiſon, preventing its adheſion to the delicate lining membranes of theſe firſt paſſages, and defending them from the violence of its attacks; while at the ſame time it promotes the diſcharge of their contents.

Theſe qualities of oil alſo render it very ſerviceable in other ſpecies of poiſon.

objected

objected the danger of its aggravating the effects of the poiſon by its ſtimulus and irritation. Though I was not in the leaſt convinced by the objection, yet, from an apprehenſion of the reflections, which might probably be made after the tragical ſcene, which ſeemed to be inevitable, I was ſtaggered in my proceeding, and wiſhed the objection had not been ſtarted. My brother, obſerving my uneaſineſs, aſked me, if ſhe ſhould ſend for Dr. Dymock. I gladly accepted this offer, as it rid me of my perplexity, and would give ſatisfaction to all concerned to have had the beſt advice. In the mean time, I plied the patient with oil and water alternately, with which ſhe vomited; but ſtill grievouſly complained of a burning heat in the ſtomach and bowels. Her breath and all ſhe vomited ſmelt very ſtrong of the camphor. Her pulſe was moderate, and not much quickened. I had now given her about a gallon more of water, and half a flaſk more of oil, when Dr. Dymock arrived. Upon informing him of all the particulars of the caſe, he, without heſitation, ordered an emetic of *Vin. Ipecacuan.* ʒ. which was immediately given: but the patient complained more and more of an inward burning heat, which made it neceſſary to ſupply her with more water before the emetic operated. It had however in a ſhort time the deſired effect, and operated plentifully both by vomit and ſtool, eſpecially the latter way. The ſtools, which continued to be diſcharged for near an hour, without any griping pain, very manifeſtly diſcovered both camphor and oil being mixed with them. The purging now began gradually to abate, and ſoon after the burning heat in the region of the ſtomach became

more

more tolerable, and inſenſibly grew better, the camphor being no longer perceptible in the breath or evacuations. Her drink now was water with the addition of a little milk.

The patient's ſpirits, which, by means of the great irritation and feveriſh tumult the cauſtic tincture had excited, kept up ſurpriſingly, now began to fail her, and ſhe was with difficulty got into bed: where, after complaining for a ſhort time only of a ſoreneſs in the firſt paſſages, ſhe lay ſweating profuſely for four hours in a very low deſponding condition.

A gentle opiate was exhibited, which took effect; and after a ſound ſleep of five hours the patient waked very eaſy, took ſome of the ſperma ceti mixture, and had another ſleep of three hours. She now found herſelf free from all her complaints; the previous ſlow fever, as well as the effects of the poiſonous tincture, being entirely carried off.

It is remarkable, that the patient found herſelf, for four days ſucceſſively, in ſo happy a ſtate of eaſe and tranquillity, as ſhe had never before experienced, and to this day enjoys a perfect ſtate of health.

Thus have I laid before you the true hiſtory of this caſe; for the ſimple relation of which I flatter myſelf I need make no apology, when I reflect, that the practice of phyſic muſt derive much greater advantages from plain hiſtories of matter of fact, and juſt deductions drawn from them, than from the moſt ingenious hypotheſes and ſpeculative theories. Theſe were of baneful influence to the progreſs of true knowlege, till your illuſtrious Society taught the world the true method of inveſtigating the laws of

nature, by obſervation of fact, experiment, and rational deduction.

The following obſervations, which this caſe naturally ſuggeſts, ſeem to deſerve our attention.

1*ſt*, That in any ſimilar accidents of ſwallowing corroſive poiſonous ſubſtances, a quick and reſolute adminiſtration of theſe ſimple bodies, water and oil, in a large quantity, ſeems to be the moſt effectual method of preventing any bad conſequences, and far preferable to the numerous boaſted antidotes, which have been handed down to us.

2*dly*, That an emetic may be more ſafely and effectually adminiſtred, and its operation waited for, after the acrimony of the poiſon hath been ſheathed and blunted, and the coats of the ſtomach defended from its attacks, by a liberal uſe of water and oil, than immediately after it is ſwallowed.

3*dly*, That as the ſlow fever and redundancy of milk, as well as the poiſon, were carried off by the copious diſcharge excited in the eaſy manner abovementioned; might we not often hope for ſucceſs in fevers occaſioned by ſimilar cauſes, plenitude and obſtruction, from plentiful evacuations, brought on after the ſame manner, by ſimple, diluent, and ſheathing medicines?

4*thly*, The camphor was undoubtedly of great ſervice in curbing the deſtructive effects of the euphorbium, by blunting its acrimony, and ſoothing the nerves into an inſenſibility of irritation, and conſequently an incapacity of ſpaſmodic affections. I have tried the *tinct. euphorb. cum camphorâ* on a horſe's

horſe's leg ſeveral times, and find it not near ſo cauſtic as without the camphor. And it is well known how much camphor involves the ſpicula, corrects the acrimony, and mitigates the effects of cantharides, ſaccharum Saturni, and rough, mercurial, and antimonial preparations.

5*thly*, To water and oil therefore we may juſtly add camphor as a powerful corrector and expeller of poiſons in general. This it probably effectuates, 1ſt, by blunting the acrimony; 2dly, by calming the nervous ſyſtem, and ſecuring it from ſpaſmodic tumult and convulſion, which may be a conſequence of its ſheathing quality; 3dly, by its extreme ſubtilty and volatility, whereby it freely penetrates the ſmalleſt receſſes of the body, and powerfully promotes a diaphoreſis. Some late inſtances of the effects of camphor in poiſonous caſes greatly confirm this account.

Theſe three ſimple bodies then, water, oil, and camphor, challenge the firſt place among the antidotes hitherto diſcovered, both for internal and external uſe, and are much more to be depended upon than any of the elaborate compoſitions calculated for this purpoſe by the ancients, as the *Theriac. Androm. Mithridat. Conf. Paulin. &c.*

LXIV. *An Account of artificial Cold produced at* Peterſburg: *By Dr.* Himſel. *In a Letter to Dr.* De Caſtro, *F. R. S. Tranſlated from the* French *by* James Parſons, *M. D. F. R. S.*

Read May 1. 1760. ON the 14th of December [1759] we had, at Peterſburg, the moſt exceſſive cold weather that ever was known, even to 205 degrees of De Liſle's thermometer. At that time Profeſſor Braun repeated Fahrenheit's experiments, in order to produce exceſſive cold by means of ſpirit of nitre combined with ſnow. He ſaw, with ſurprize, the quickſilver fall conſiderably in the thermometer, and deſcend even to 470 degrees at laſt: there the quickſilver remained fixed in the open air for the ſpace of a quarter of an hour, and did not begin to riſe till it was carried into a warm room. He repeated the ſame experiment, firſt with the ſame, and then with another thermometer, with the ſame ſucceſs. The immobility of the quickſilver made him conjecture, that it might be frozen, or become a ſolid body. But as Mr. Braun had not broken the glaſſes, he could only at that time form a conjecture. On the 17th he produced, again, cold equal to that of the 14th; but on that day there was no experiment made; and Mr. Braun communicated his diſcovery at a meeting of the academy. On the 25th of December in the morning, between nine and ten, De Liſle's thermometer was at the 199th degree of cold; and Mr. Braun, as well as Profeſſor Æpinus,

Æpinus, then repeated this experiment. As ſoon as the former had obſerved the quickſilver immoveable in the thermometer, he broke the glaſs; and he found, to his amazement, the quickſilver frozen, but not intirely; for in the middle of the glaſs ball there was a ſmall portion yet remaining fluid. Mr. Æpinus's thermometer fell, with extreme rapidity, almoſt to the 500th degree, and in breaking the glaſs from below, he found the quickſilver contained in it abſolutely frozen. Both the gentlemen found, that the quickſilver, thus rendered ſolid, bore hammering and extenſion, like other metals; but being aftewards expoſed to the open air, it recovered its former fluidity in a little time.

Mr. Æpinus went ſomewhat farther, in order to examine the quickſilver, when it was made ſolid. He poured quickſilver into a glaſs tube as thick as one's finger, cloſed at bottom, but open at top. The quickſilver in this cylinder, which was about one inch and half long, froze in three quarters of a minute; and he obſerved, that it became ſolid, perfectly reſembling other metals, except iron: it continually contracted, and its ſurface, which was at firſt pretty high, ſoon ſunk very low. This cylinder of frozen quickſilver ſunk to the bottom of fluid quickſilver, in the ſame manner, as is obſerved of other metals, except iron. We know the contrary happens with regard to water frozen and other fluids, which extend as they become ſolid, and their ice ſwims in the fluid matter, of which they were produced.

On the 26th of December in the morning, between nine and ten, the cold became extremely ſharp at 211 degrees, and ſuch as exceeded the greateſt

degree of artificial cold fixed by Fahrenheit; for 40 degrees below *zero*, in Fahrenheit's thermometer, is equal to 210 degrees of that of De Liſle.

Mr. Braun repeated this experiment again, exactly with the ſame ſucceſs with that of the day before. The counſellor and profeſſor Lomonoſſow made the ſame experiment on the ſame day; and by means of aqua fortis the cold came to 495 degrees. He then poured in ſpirit of common or ſea ſalt, and the quickſilver fell down in the thermometer to 554 degrees; and in taking the thermometer from the mixture, the quickſilver continued to fall in the open air to the 552d degree. He threw yet into the glaſs a little more ſnow, pouring on it ſome oil of vitriol, and ſuddenly the quickſilver fell to 1260 degrees. Then he broke the ball, and found the mercury changed to a ſolid body. The quickſilver, which yet remained in the tube, was alſo become ſolid, and appeared like a looſe ſilver wire, attached to the ball, which was flexible every way. He gave the ball of quickſilver ſeveral blows with a turned ax, and it became flat like a half-ruble, or Engliſh half-crown; but receiving thereby ſome cracks, it diſſolved in about 20 minutes. Theſe experiments were made when the air was at about 208 degrees of cold.

Mr. Kraſe, Mr. Zeicher, and the firſt apothecary Model, and again Mr. Æpinus, repeated the ſame experiment with the very ſame ſucceſs. It is to be obſerved, that at the ſecond experiment by profeſſor Zeicher, on the 31ſt of December, as the air was then only at the 183d degree of cold, in taking the thermometer from the mixture, in which the quickſilver was at 300 degrees, it fell yet 100 degrees

grees more in the open air. The teſtimony of ſo many philoſophers, each of whom had reſpectively made the experiment, will, no doubt, be ſufficient to prove the truth of it. But in order to remove all doubt about it, it muſt be remarked, that diſtilled quickſilver only was made uſe of in every experiment; nay, in ſome, the quickſilver was revivified from ſublimation. There can therefore be no ſuſpicion, that what they uſed was impure, or mixed with any heterogeneous matter. This appears to have happened to Mr. De Liſle de la Croyere, when he ſays, that in Siberia he found the quickſilver congealed in the barometer: and even his papers, which are in the academy, ſhew that he made a miſtake in his remarks; for, according to them, the mercury became ſolid as ſoon as it fell to about 195 or 200 degrees: but the mercury, which is pure, does not congeal at that degree; for otherwiſe it would not be very extraordinary with us to ſee it take a ſolid form, becauſe it is not rare to find the cold at this degree here. We may believe, that the quickſilver uſed by Mr. De la Croyere was impure, and therefore might ſooner become an amalgama than pure mercury.

Now there are two things we cannot reaſon upon with any certainty: As to the hardneſs of the quickſilver congealed, it appeared to have had, in ſome eſſays, leſs hardneſs than lead, and in others more: alſo we cannot be very ſure of the degree of cold, by which the mercury is conſolidated. The greateſt part of the experiments agree in this, that the quickſilver becomes ſolid, when it falls in the thermometer to 500 degrees, more or leſs. Nevertheleſs, they do not ſo ſufficiently agree as to deduce any thing certain about it.

Although

Although in the experiment made by Mr. Lomonoſſow the quickſilver fell to 1260 degrees, this philoſopher nevertheleſs ſays, that he could not ſufficiently obſerve, in his hurry, whether the ball might not have received ſome crack, and the quickſilver thereby perhaps might have had liberty to fall the lower, which otherwiſe would not have happened; for the ſame thing happened to Meſſ. Braun, Zeicher, and Æpinus, that the balls of their thermometers were cracked and broken. By the experiment of Mr. Æpinus, made on the 25th of December, in which the quickſilver fell ſuddenly in the thermometer, and the cylinder of quickſilver of the thickneſs of one's finger becoming ſolid ſo quickly, it may be obſerved, that the degree of cold then produced ought to exceed 300 degrees. Nevertheleſs, whatever the degree be, we cannot determine how the common thermometer ceaſes to be of uſe as ſoon as the mercury begins to become ſolid.

Here follows an account of the manner, in which theſe experiments may be made, that other philoſophers may be capable of repeating them. It is therefore to be obſerved, that it is neceſſary to uſe fuming ſpirit of nitre, or of ſuch as is evaporated till the fumes become red; for the common aqua fortis, which is uſed, had not the deſired effect. Mr. Æpinus has found, that this experiment is very eaſily and ſpeedily made in the following manner. Take ſpirit of nitre, cooled as much as poſſible, and with it half fill a wine-glaſs, throwing in as much ſnow at the ſame time, and ſtirring it till it becomes of the conſiſtence of pap: then you have almoſt in an inſtant the neceſſary degree for the congelation of the quickſilver.

quickſilver: not only Mr. Æpinus uſed this method, but alſo Meſſ. Kraſe, Zeicher, and Model, who followed Mr. Æpinus, and found it the moſt convenient method. Now, in reflecting upon the procedure of other philoſophers, eſpecially of Meſſ. Muſchenbroek and Reaumur, for producing artificial cold, by the commixtion of ſnow with aquafortis, as the former has mentioned in his edition of the experiments of the academy of Florence, tom. i. p. 174. and Mr. Reaumur, in the memoirs of the academy of ſciences of Paris for the year 1734. it is aſtoniſhing how it happens, that theſe learned men have not obtained, by a great deal, the degree of cold produced by the gentlemen of the academy of Peterſburg; for their manner of making the experiments does not ſeem to differ much from that of Mr. Braun, as to what relates to any eſſential circumſtances, nor from the manner mentioned before, ſo as to hinder them from producing effects nearly equal. Perhaps they may, in time, and by continued reſearches, be able to find out the circumſtances, that prevented their ſucceſs in the experiments of theſe great men: it may be, becauſe the ſpirit of nitre was not endowed with its proper quality. In fine, I muſt further obſerve, that a certain degree of external cold appears abſolutely neceſſary to the experiment. Mr. Æpinus, who made it the 28th of December, in a room where De Liſle's thermometer ſhewed 122 degrees of cold, cooled the ſpirit of nitre in liquifying ſnow to 150 degrees, and the ſnow, which they uſed, came to the ſame degree; in making the mixture, the reſult was an augmentation of cold to 300 degrees. It muſt then happen, that they had obtained the ſurprizing

degree neceſſary to congeal the mercury; which Mr. Zeicher alſo at length obtained; the degree of cold of the air being the 175th degree of De Liſle's thermometer, or the 30th of that of Fahrenheit.

LXV. *An Account of a complete Luxation of the Thigh Bone, in an adult Perſon, by external Violence*; *by Mr.* Charles White, *Surgeon, at* Mancheſter. *Communicated by* George Lloyd, *Eſq*; *F. R. S.*

Read May 1, 1760.

AS Robert Hogg, (a farmer in Clyſton, about four miles from Mancheſter) a ſtrong, robuſt, middle-aged man, was taking a load of wheat from off a horſe, on the 20th of March 1759, his foot ſlipping, he fell backwards; his breech upon the pavement, and the load of wheat upon his belly and thighs. The ſervants carried him into the houſe, and laid him upon a bed, where he remained in the moſt racking torture, when I came to him, which was about two hours after the accident happened. I found his right buttock as large again as the other; the knee and foot of the ſame ſide turned inwards; and the thigh much ſhortened. Upon endeavouring to make the thigh perform its rotatory motion, there was not the leaſt crackling to be heard. This convinced me, that the head of the bone was thrown out of the acetabulum; and, upon examination, I could diſtinctly feel it under the glutæi muſcles: to which ſituation of it, and not to any bruiſe,

bruiſe, I was now ſatisfied that the ſize of the buttock was owing.

I ſoon reduced it, by the following eaſy, and very ſimple method.

Some napkins being firſt lapped round one of the poſts at the foot of the bed, to prevent its galling him, I ordered the patient to be laid upon his back, with one leg on each ſide the poſt, and then directed three or four aſſiſtants to pull at the diſlocated limb, the poſt now placed to his groin being a fixed point to pull againſt. Whilſt they were making this extenſion, I clapped my left hand upon the head of the bone, to help it into its place; and, at the ſame inſtant, with my right hand, turning the knee outwards, threw the bone into the ſocket, with the greateſt facility imaginable, but with ſuch an uncommonly loud noiſe, as greatly aſtoniſhed all who were preſent.

He was perfectly eaſy from that moment; the enlargement of the buttock entirely ſubſided. In a fortnight, he was able to move about, without aſſiſtance; and in two months afterwards walked as far as Mancheſter, being then quite ſound; and the limb, that had been diſlocated, of the ſame length with the other.

Remarks.

Both antients and moderns have fallen into great errors, in regard to the treatment of accidents, that have happened to the hip joint. The antients, who, for want of frequent opportunities of diſſecting bodies, were ignorant, that the neck of the femur was often broken, always imagined it to be luxated. Their

patients were, therefore, ſometimes tormented, (in hopes of a reduction) without any advantage; and this want of ſucceſs made the ſurgeons, at other times, abandon their patients, when they might have been relieved. The moderns have fallen into a contrary extreme, but attended with as bad conſequences. Boerhaave, in particular, was of opinion, that there never was a diſlocation of the thigh bone by any external violence, but that the head of it was commonly broken off at its neck, near the great trochanter. The opinion of ſo learned a man has had ſuch weight with the generality of the profeſſion, that it has been taken for granted, that, in theſe caſes, the neck of the bone was always broken: conſequently, the reduction was ſeldom attempted, and the unfortunate patients remained cripples during the reſt of their lives. But the point is now, I think, cleared up, beyond the poſſibility of a doubt.

In the ſecond volume of the memoirs of the royal academy of ſurgery at Paris, there are two caſes related, to ſhew the reſources of nature, where luxations of the thigh bone have not been reduced. Here it appears, (from examination after death) that, in the firſt caſe, the bone was thrown out upwards and outwards, the cotyloid cavity greatly diminiſhed in ſize, and its figure changed from round to oval. The head of the femur was received into another cavity formed upon the os ileum, under the glutæus minimus, which ſerved it as a capſula, to ſecure it within this preternatural cavity. This accident was occaſioned by a fall, when the patient was a child. She was afterwards able to walk about, though ſhe continued a cripple to the time of her death, which

happened

happened at the age of ſixty-eight. In the other caſe, the bone was luxated downwards and inwards, and the head fixed upon the foramen ovale.

There is a caſe too related, and very well atteſted, in the Edinburgh eſſays, philoſophical and literary, vol. 2d, of a man at Worceſter, who had the head of the bone thrown out of the acetabulum, and lodged in the groin. It was with ſome difficulty reduced, and the man ſuffered no other inconvenience, than that of the leg's being about a quarter of an inch longer than the other.

To theſe, let me add, that, about thirty years ago, my father was ſent for to a man, who had luxated his thigh bone three or four days before. The head of it lay in the groin, which the ſurgeon, who was firſt employed, did not diſcover. However, it was immediately replaced, and the patient recovered the uſe of his limb, in a very ſhort time.

From what I have ſaid, I would by no means have it concluded, that the neck of the bone is not ſometimes broken, or that it is not even oftener broken than luxated: but, from the caſe, which has fallen directly under my notice, joined to thoſe, which I have above recited, I think it muſt appear very clear, that it has been frequently luxated, and that two different ways.

C. White.

LXVI. *Con-*

LXVI. *Conjectures upon an inedited* Parthian *Coin. By the Rev.* John Swinton, *B. D. of* Chriſt-Church, Oxon. *F. R. S.*

To the Rev. Thomas Birch, *D. D. Secretary to the Royal Society.*

Good Sir,

Read May 1, 1760. I Some time ſince met with a Parthian coin, nearly of the ſize of the middle Roman braſs, that ſeems to merit the attention of the curious. For though the piece itſelf is in very indifferent conſervation, yet it has not been hitherto publiſhed, and may perhaps ſerve to illuſtrate an exceeding rare coin, with a draught of which the learned world has been obliged by M. Bouterouë. As therefore it may poſſibly be deemed not altogether unworthy the notice of the Royal Society, I have taken the liberty to trouble them with a few conjectures upon it; eſpecially, as they have done me the honour to order the publication of two papers, a few years ſince, upon ſimilar ſubjects. To this I have likewiſe been farther excited by part of a letter, written lately to my worthy friend the Reverend Mr. John Nixon, a learned member of the Society, by Sig. Abate Venuti; who therein mentions the famous F. Corſini's approbation of my remarks upon Monneſes's coin, inſerted in the fiftieth volume of the *Philoſophical Tranſactions*, excepting that he ſuſpected the *Victory* on the reverſe to allude to the Parthian monarch's devaſtation of the neighbouring provinces, and uſurpation of the

Parthian

Parthian throne. I have therefore endeavoured to remove the ſuſpicion F. Corſini has entertained, and attempted to prove, that the impreſſion of this *Victory*, ſo utterly repugnant to the Parthian genius, muſt have been occaſioned by ſome remarkable advantage, gained by the Parthian forces over the Romans. In order to which, I have at leaſt in ſome meaſure evinced the Romans to have adorned with ſuch a *Victory* the reverſes of their coins, not only after a battle had been won, as F. Corſini would ſeem to maintain, but likewiſe on certain occaſions after other proſperous military exploits. It is alſo here rendered probable, that the *Victory* was impreſſed on Monneſes's medal, in alluſion to the manner in which the Romans treated the Parthians, on their coins, after any favourable military event; that F. Corſini is not quite conſiſtent with himſelf, the reaſon by him aſſigned for differing from me, on the principle he has advanced, being ſubverſive of his own opinion; and that, as the reverſes of the other Parthian medals never exhibit a *Victory* in the Roman taſte, the coin before me could have been ſtruck only to perpetuate the memory of ſome ſignal atchievement, performed by the Parthian arms againſt the Romans.

If the conjectures now ſubmitted to the conſideration of the Royal Society ſhould meet with the approbation of that moſt learned and illuſtrious body, they will undoubtedly be allowed to throw ſome light on the ſingular and valuable medal above-mentioned, publiſhed by (1) M. Bouterouë, never hi-

(1) *Recherches Curieuſes des Monoyes de France &c. Par* Claude Bouterouë, p. 33. A Paris, 1666.

therto

therto explained. I therefore judged it not improper to tranſmit you an accurate draught of this coin, as well as one of the medal here ſo particularly deſcribed; [*Vide Tab.* XVII.] which may probably be the more acceptable to many, who have a taſte for this kind of literature, as M. Bouterouë's book itſelf cannot now without great difficulty be found.

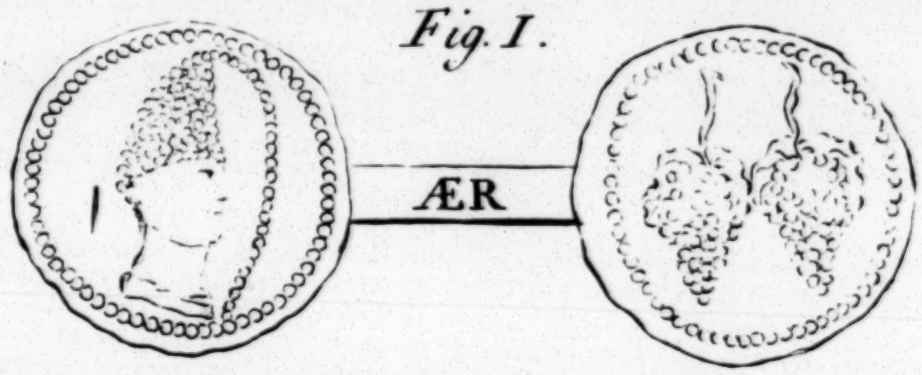
With regard to the language of the paper in which my remarks are contained, I ſhall only beg leave to hint, that it is underſtood by all who are proper judges of the performance. For this therefore I ſhall offer no other apology, than that the letter from Sig. Abate Venuti to Mr. Nixon, which occaſioned it, is penned in the Latin tongue; and that the famous F. Corſini, the removal of whoſe doubt or ſuſpicion was one of the principal objects I had in view, writes for the moſt part at leaſt in the ſame language. I might however add, that many learned foreigners, who are particularly pleaſed with ſuch diſquiſitions, are much better acquainted with Latin than any other tongue, except their own; and that it were to be wiſhed the uſe of this noble language, in the republic of letters, were more general than it at preſent (2) ſeems to be. You will give me leave only farther to ſuggeſt, that there can be no impropriety in communicating to the Royal Society the following remarks, the (3) paper they are intended to ſupport not only authorizing but even requiring ſuch a com-

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(2) *Nov. Act. Eruditor. Lipſ. Menſis Maii* A. 1755. p. 252.
(3) *Philoſoph. Tranſact.* Vol. L. p. 184—187.

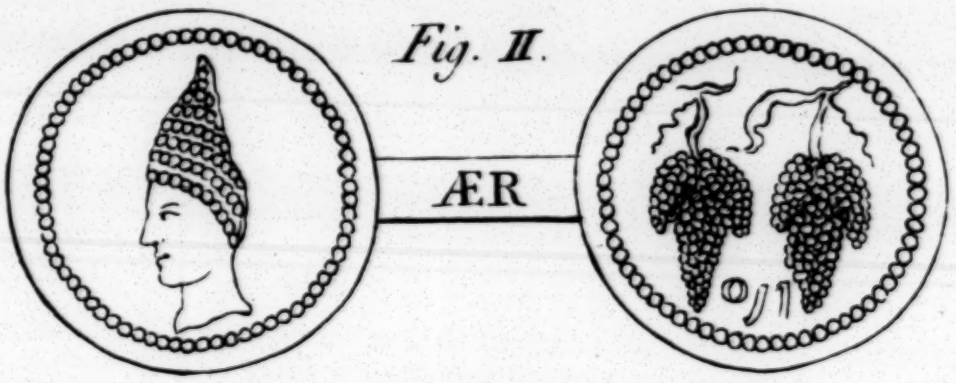
munication;

Fig. I.



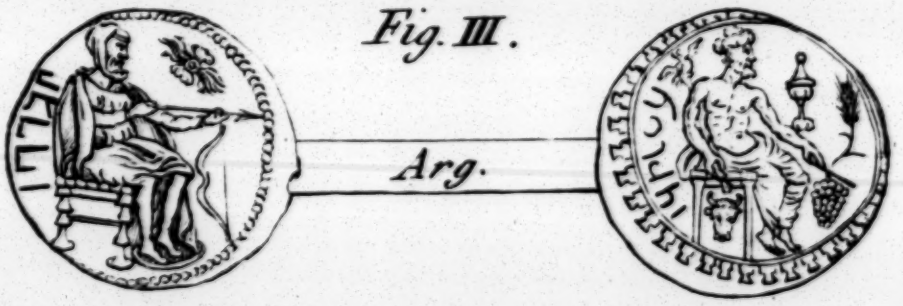
Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.

Fig. II.



Apud D. Bouteroue, *p.* 33.

Fig. III.



Apud Henricum March *Armig.*

I. Mynde fc.

munication; and to aſſure you that I am, with the moſt perfect attachment and reſpect,

SIR,

Your moſt faithful,
and moſt obedient,
humble ſervant,
J. Swinton.

Chriſt-Church, Oxon.
Apr. 24th, 1760.

In Nummum Parthicum hactenus ineditum conjecturæ.

I.

NUmiſma planè incognitum, quod vulgavit (1) D. Bouterouë, in adverſâ parte regis cujuſdam imberbis exhibet caput, tiarâ margaritis ſive unionibus fulgente ornatum; in averſâ verò binos racemos, ut et trinas inuſitatâ formâ literas, quarum poteſtas etiamnum ignoratur. Aliud item haud parùm detrimenti à tempore perpeſſum, cujus ectypum Regiæ Societati jam viſendum ſiſto, capite regis tali mitrâ

(1) *Recherches Curieuſes des Monoyes de France &c. Par* Claude Bouterouë, p. 33. A Paris, 1666. Si *Gamma* pone Vologeſis III. caput Gazam, vel Gazacam, Mediæ Atropatiæ urbem, indigitet, ut exiſtimat Vaillantius (1); trina elementa incognita, in nummo Bouteriano conſpicua, eandem forſan urbem deſignent. Quod et ipſæ eorundem formæ ſuadere videntur. Primum enim *Gimel* Phœnicium figurâ refert, ſecundum *Zain* Chaldaico et Palmyreno non eſt abſimile, tertium verò ipſiſſimum Phœnicium *Ain* ſe facilè fatetur. *Ain* autem (2) nunc *Aleph*, nunc *Gimel* (3) ſonuit. Hinc igitur vox Gaza, Straboni (4) uſitata, Gazag, vel Gazaca, quæ Stephano (5) Byzantino magis placuit, fortaſſe ſit enata.

(1) J. F. Vaill. *Arſacid. Imper.* p. 366. Pariſiis, 1728. (2) Boch. *Chan.* p. 812, 813. Francof. ad Mœn. 1681. (3) Id. ibid. p. 561. (4) Strab. Lib. xi. p. 523. (5) Steph. Byzant. in voc. ΓΑΖΑΚΑ.

ſeu tiarâ conſpicui nobilitatum, ſupellex mea antiquaria ſuppeditat. Hoc ante caput ſceptrum oſtentat, pone autem figuram, quæ Græcum elementum *Gamma* referre videtur. Cætera nummus noſter Bouterouiano non diſſimilis jure haud iniquo cenſendus. Quare altero aliquantulum illuſtrato, nonnihil etiam lucis alteri forſan aſpergatur.

II.

Tiaram quod attinet, noſtro in numiſmate depictum, eum è rectis unum eſſe, quos, Xenophonte (2) teſte, Cyrus accepit à Medis, et ad ſucceſſores tranſmiſit, ſummæ majeſtatis inſignia, ita ut nemini liceret gerere, niſi regibus, ex auctoribus hîc laudatis abundè fit perſpicuum. " Solos (ait (3) V. C. Theophilus Sigefridus Bayerus) " reges geſſiſſe ad- " firmant nobis Ariſtophanes et ejus Scholiaſta de " Perſis, Joſephus autem et Dio Pruſæus de Parthis." Pergit Cel. Bayerus, " Quos igitur (tiaras) dicemus " rectos niſi eos, qui non lunatum in orbem circum " tempora flecterentur, ſed rigidiores eſſent apices et " altiores?—Neque auſi ſunt (ſi eundem audiamus Bayerum) " Edeſſeni reges iis uti, potentia et ma- " jeſtate inferiores Parthis." Quandoquidem igitur hic nummus ante everſum ab Artaxerxe Parthorum imperium videtur procuſus, ac proinde ad Perſas, ſaltem poſteriores, vix referendus; eum à Parthiæ quodam rege fuiſſe percuſſum, etſi tiaræ figura parti adverſæ inſculpta complurium Parthicorum formæ non ad amuſſim congruat, omninò certè par eſt arbitrari.

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(2) Xen. *Cyrop.* L. viii. c. 3.

(3) Theoph. Sig. Bayer. *Hiſt. Oſrhoen. & Edeſſen. &c.* p. 135, 136. Petropoli, 1734.

III.

III.

His ritè perpenſis, tiaram nobis Parthorum regem indigitare, cordatum quemque vix inficias exiſtimo iturum. Quo conceſſo, Vologeſem III. numiſma noſtrum cudiſſe, indicio erunt, forſan certiſſimo, in priori facie ante caput ſceptrum et pone litera *Gamma*, ſi modò figura ſuperiùs memorata pro iſto elemento commodè ſatis accipi poſſit. Etenim hujuſce principis nummum, à (4) Cl. Vaillantio publici juris factum, ſive typum, ſive modulum reſpiciamus, noſtrum plurimùm referre, vel cæcutienti ſat clarè innoteſcat. Nil interim mirandum, quòd caput regis imberbis, ſeu potiùs mediocriter barbati, antica primùm oſtentaverit; cum (5) Mannus, regulus finitimus Parthis, in nummulo ſine barbâ depingatur, et nonnulli Parthorum (6) reges (quorum numero ipſe forſan Vologeſes III. accenſeatur) mediocriter tantùm barbati in numiſmatis interdum conſpiciantur.

IV.

Quod ad binos racemos ſpectat, quibus pars nummi noſtri poſterior cernitur ſignata, id quoque quicquid ſit ſymboli Parthorum regem ante oculos ponere fortaſſe reperietur. Numiſma enim argenteum majori formâ penes ſpectatiſſimum virum, Henricum March, armigerum, à V. C. Arthuro Pullinger ex Oriente

(4) J. F. Vaill. *Arſacid. Imper.* p. 364—369. Pariſiis, 1728.

(5) Nicol. Fran. Haym Roman. *Del Teſor. Britan.* Vol. Second. p. 57, 58. In Londra, 1720. *Numiſm. Antiqu. Collect. à* Thom. Pembr. et Mont. Gom. Com. P. 2. T. 72. Wiſe *Epiſt. ad V. Cl. Joannem Maſſon de num. Abgari Regis.* Oxon. 1750.

(6) J. F. Vaill. *Arſacid. Imper.* p. 31, 58, 105, 321, 364, 366, &c.

ſecum in Britanniam reportatum, in anticâ typum oſtentat, Parthico (quo complures nummi (7) inſigniuntur) non diſſimilem: principem nempe ſellæ inſidentem, manibuſque ſagittam geſtantem, ante cujus pedes viſitur arcus; in poſticâ verò ſenem, ſiniſtrâ tenentem racemum. Si igitur numiſma noſtrum ad Vologeſem III. pertinuiſſe autumemus, à vero forſan non multùm abludemus. Interim ſi cordatus quiſpiam aliter ſenſerit, integrum ſit ipſi ſuâ gaudere ſententiâ, eandem quoque nobis largiatur veniam.

V.

Si ſuperiùs annotatis ſuffragentur eruditi, et a Vologeſe III. nummulus noſter habeatur revera percuſſus, rogitet forſan aliquis, " quî fit ut princeps iſte æreos " cuderit nummos, cum argentei ferè ſoli (ſi ex iis " qui nobis ſuperſunt hâc de re judicium faciamus) " ſub ejus in imperio anteceſſoribus prodierint?" Cui reſpondeamus licet, " Monneſis nummus, te- " tradrachmarum magnitudine (8), à Cl. Vaillantio " evulgatus, pariter ac noſter, æreus eſt; quin et " alium Vologeſis II. rariſſimum ad æreorum mediæ " formæ numerum idem adſcripſit (9) Vaillantius. " Quidnì igitur Vologeſis III. numiſmata interdum " ex ære cernantur conflata? Hoc enim factum " fuiſſe propter res Parthiæ difficillimas, eo imperii " clavum tenente, ſummamque argenti penuriam, " cum jam laudato (10) Vaillantio arbitramur."

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(7) J. F. Vaill. ubi ſup. paſſ.
(8) J. F. Vaill. *Arſacid. Imper.* p. 341.
(9) Idem ibid. p. 345.
(10) Id. ibid. p. 341.

VI.

VI.

Parthorum res ſub Vologeſe III. haud parùm fuiſſe turbatas, bello iis à Severo tunc temporis illato, (11) ex Herodiano, Spartiano, et Dione abundè conſtat. Anno enim Arſacidarum æræ vulgaris 456°. vel 457°. ſeu potius, ſi meam eatur in ſententiam (12), 429°. Severus Parthos inopinatò eſt aggreſſus; quin et cæſis primò *quicunque obvii ei ſunt facti*, non *quicunque reſtitiſſent*, ut Herodiani (13) verba, πάντα τε ἁπ ἐμπίπλοντα, perperàm Latinè vertit Angelus Politianus, regiam Cteſiphontem trajecto Tigri cepit, atque militibus diripiendam conceſſit. Illinc ipſo rege cum paucis tantùm equitibus elapſo, theſauris illius ornatuque omni et ſupellectile potiti victores regrediuntur. Quanquam autem, proſperè ſuccedentibus rebus, literas ad Senatum Populumque Romanum de ſe magnificas dederit, ac depictas in tabulis pugnas, victoriaſque ſuas, ſi Herodiano (14) fides ſit habenda, expoſuerit Severus, nihil non honorum decernente Senatu, et ſuper his cognomina imponente gentium devictarum; " ullam (15) tamen, eodem " anno, Romanos inter et Barbaros revera interfuiſſe " pugnam," ex iſto hiſtorico haud facilè colligamus. Neque ab Herodiano hâc in re diſſentit Spartianus (16), qui nullius Severum inter et Vologeſum prælii,

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(11) Herodian. Lib. iii. Æl. Spartian. in *Sever.* Dio, Lib. lxxv. p. 854.

(12) *Philoſoph. Tranſact.* Vol. L. p. 183—187.

(13) Herodian. ubi ſup. c. 30. Oxon. 1678.

(14) Idem ibid. c. 31.

(15) Venut. *ad V. C. Joannem Nixonum, A. M. & R. S. S. Epiſt.* Vid. etiam *Act. Philoſoph. Anglican.* Vol. LI. p. 205.

(16) Æl. Spartian. ubi ſup.

dicto

dicto anno commiſſi, uſquàm meminit. Quod quidem ſilentium calculo ſuo egregiè confirmat Dio (17), cujus ſic ſe fermè habent verba: " Item Seluciam et " Babylonem *deſertas celeriter occupavit*, ac paulo " poſt Cteſiphontem; quam urbem diripiendam re- " liquit militibus, et in eâ cædem maximam fecit, " cepitque ſuperſtitum hominum ad centum millia. " Vologeſum tamen non eſt inſecutus, neque Cteſi- " phontem retinuit; ſed tanquàm ob id ſolum ve- " niſſet, ut eam &c." Undè veriſmile admodum videtur, nullam Romanos inter et Barbaros hâc fortuitâ in Parthiam expeditione interfuiſſe pugnam; cum Seleuciam et Babylonem ab hoſtibus fuiſſe *deſertas*, et à Severo *celeriter occupatas*, celeberrimus ille ſcriptor diſertè monuit. Celeberrimus, inquam, ille ſcriptor; cum Dioni Severi comiti et familiari omninò fidendum, præſertìm ſi ſuo teſtimonio dicti duo hiſtorici, Spartianus et Herodianus, ſcriptis ejus etiam fidem adſtruxerint. Aſt quanquam in ſuâ ad Senatum epiſtolâ prælia quædam tantummodò commentus ſit Severus, *Victoriam* in averſâ parte *Gradientem* præ ſe ferentes (18), denarios tamen, in perpetuam rei memoriam, ſignari juſſit. Undè quodammodò patet, Romanorum imperatores *Victoriam* in nummis *Gradientem* interdum ſcalpſiſſe, licet nulla " in- " ter eos gentemque devictam interfuiſſet (19) pugna." Quod et alio forſan exemplo, ex hiſtoriâ Romano-Parthicâ petito, clariùs eluceſcet. In belli adverſus Parthos ab Antonino Caracallâ geſti memoriam, prin-

(17) Dio, Lib. lxxv. p. 854.
(18) J. F. Vaill. ubi ſup. p. 361.
(19) Venut. ubi ſup.

ceps

ceps iſte (20) nummos argenteos percuſſit, *Victoriæ Parthicæ* typum exhibentes. Incruentam tamen à Parthis reportavit victoriam. Etenim, " ingenti cæde " Parthorum factâ, prædâque et hominibus magnâ " copiâ captis, diſcedit, teſtante Herodiano (21), " μηδενὸς ἀυτῷ ἀνθεςῶτος, *reſiſtente nullo*, viciſque " et urbibus incenſis, datâ poteſtate militibus ra- " piendi, quod quiſque poſſet, aut vellet." Cum Herodiano item, hâc in re (22), concinit Dio; ei enim ſi fidem tribuamus, Antoninus regionem, quæ Mediam attingit, in quam de improviſo irruerat, magnâ ex parte vaſtavit, multa caſtella diruit, Arbela ſubegit, ac ſepulchra regum Parthorum, abjectis oſſibus, ſubvertit. " Verum (23), ait hiſtoricus, *quia* " *Parthi cum eo non venerunt ad manus*, nihil de re- " bus tunc geſtis ſcribi à me eximium poteſt."

VII.

Sed et ſententiæ noſtræ hîc in medium prolatæ calculum quoque ſuum appoſuit præſtantiſſimus Spanhemius. Inter alia enim huc ſpectantia eruditè annotavit Vir (24) Clariſſimus, " VICTORIAM PAR- " THICAM, cum in M. Antonini ac L. Veri, tum " Severi, Caracallæ, ac Macrini nummis legi, immo " in Severi nummis etiam VICTORIAM PAR- " THICAM MAXIMAM: à quibus utique prin- " cipibus *bella feliciter contra Parthos geſta* me-

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(20) J. F. Vaill. ubi ſup. p. 375.
(21) Herodian. ubi ſup. Lib. iv. c. 20. p. 163. Edit. Oxon. 1678.
(22) Dio, Lib. lxxvii. p. 881.
(23) Idem ibid.
(24) Ez. Spanhem. *De Præſtant. & Uſ. Numiſm. Antiquor. Vol. Alter.* p. 635. Amſtelædami, 1717.

" morant

" morant Romanarum rerum ſcriptores, ac firmant " inſuper, cum iidem nummi, tum additum in iis " PARTHICI, et quidem ſubinde PARTHICI " MAXIMI, cognomentum." Notatu hîc non indignum, numiſmata Romana VICT. PART. vocibus dimidiatis in averſâ parte exornata *Victoriæ* itidem figuram præ (25) ſe ferre. Undè omninò patet, Romanos in nummis VICTORIAM PARTHICAM, auctore Cel. Vir. Ezech. Spanhemio, ſcalpſiſſe (26), ſi *bella feliciter contra Parthos*, vel prælio ſecundo, vel alio quoquo modo, *gererentur*.

VIII.

Hæc de monetam *Victoriâ* ſignandi more, Romanis diu uſitato, dicta ſunto. Ad Parthos quod attinet, ex nummis talem apud eos conſuetudinem (27) non invaluiſſe conſtat. *Victoria* igitur, ſive potiùs *Victoria Gradiens*, Latinum ad modum inſculpta, certiſſimo eſt indicio, Monneſen numiſma hanc oſtentans (28), à me fuſiùs olim atque uberiùs explicatum, in rerum contra Romanos feliciter geſtarum memoriam, utque iis par pari referret, præſertìm ob ſignatam ſimili typo pecuniam à M. Aurelio Antonino et L. Vero victoribus (29),

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(25) J. F. Vaill. ubi ſup. p. 333, 375, 382, &c. Franciſc. Mediobarb. Birag. *Imperator. Romanor. Numiſm. &c.* paſſ.

(26) Ez. Spanhem. ubi ſup.

(27) J. F. Vaill. *Arſacidar. Imper.* Nicol. Fran. Haym Roman. *Del. Teſor. Britan.* Vol. Second. p. 30—37. Muſ. Theupol. p. 1221, 1222. *Numiſm. Antiqu. &c.* Thom. Pembr. & Mont. Gom. Com. &c. P. II. T. 76. Claude Bouterouë, ubi ſup. aliique Scriptor. paſſ.

(28) *Philoſoph. Tranſact.* Vol. L. p. 175—188.

(29) J. F. Vaill. ubi ſup. p. 333.

percuſſiſſe.

percuſſiſſe. At prælium exinde tunc temporis Romanos inter et Parthos fuiſſe commiſſum, propter rationes ſuperiùs allatas, haud æquo jure colligamus. Quare leviter ſaltem lapſus videtur (30) V. C. Edvardus Corſinus, (31) " ubi dubitat, ad epocham " ſtatuendam, de *Victoriâ* in poſticâ (regis Mon-

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(30) V. C. Edv. Corſin. apud Venut. in *Latin. ad Joan. Nixonum Epiſt.*

(31) Perpenſo (quod ſequitur) Cl. Nixoni apographo, ab eo mecum quàm humaniſſimè communicato, de vero hîc ſenſus acumine cordatus lector liquidiùs judicare poſſit. " Cl. Corſino doc- " tiſſimas Cl. Swintoni conjecturas de nummo Monneſis Partho- " rum regis communicavi: ille vero comprobavit, admiratuſque " eſt ſolertiam authoris, ingeniique acumen; tantummodò ubi " *dubitat*, ad epocham ſtatuendam, de *Victoriâ* in poſticâ ex- " culptâ, cum eo anno nulla interfuerit pugna inter Romanos et " Barbaros: ſed cum Monneſes regnum uſurpaverit, legitimoſque " ſpoliaverit hæredes, vicinaque devaſtaverit regna, nil mirum ſi " *Victoriam* in nummis inſculpſerit, atque ad Barbaricam, non " ad Romanam, referri debet." Hinc abundè conſtat, Corſinum, non me, ut voluit Cl. Nixonus, " ad epocham ſtatuendam, de " *Victoriâ* in poſticâ exculptâ dubitâſſe." Nemini enim non liquet, meam de verâ Arſacidarum ærâ, hujuſce *Victoriæ* typi ope, feliciùs enodandâ ſententiam, quanquam immeritò, in dubium vocâſſe Cl. Corſinum; " cum eo anno nulla, ſi illum audi- " amus, interfuerit pugna inter Romanos et Barbaros." En cauſa, cur, " ad epocham ſtatuendam, de *Victoriâ* in poſticâ exculptâ " dubitaverit" Corſinus, ab ipſo Corſino, vel, ſi malimus, Venuto, in medium prolata. Quinimò *Victoriæ* figurâ, in hoc Monneſis numiſmate ſcalptâ, æram Parthorum Corſinianam (vel invito Corſino) ſine ullâ dubitatione firmandam duxi, ut ex noſtris *Actis Philoſophicis* omninò fit perſpicuum. Voces igitur *tantummodò ubi dubitat* minùs rectè *but excepted a little to that part, where he* [*Mr. Swinton*] *expreſſes a doubt*, ſiquid judico, Anglicè vertit Cl. Nixonus. Neque meo ſolo, hâc in re, nixus ſum judicio; cum præcellentes, quotquot conſului, eruditione amici, inter quos eminet reverendus ac doctiſſimus vir, Joſephus Sanford, S. T. B. Collegii Balliolenſis Oxon Socius, mihi certatim adſtipularentur. *Philoſoph. Tranſact.* Vol. L. p. 185.

" nefis nummi) exculptâ, cum eo anno nulla " interfuerit pugna inter Romanos et Barbaros;" tametſi meam hujuſce numiſmatis explicationem, quod ſummo mihi honori duco, teſte Cl. Venuto (32), comprobavit.

IX.

Sed et minùs ſibi conſtare, ſi non apertè ſecum pugnare, quoque videtur vel Cel. Corſinus, vel doctiſſimus Venutus; cujus huc ſpectantia, in pererudità ſuâ ad V. C. Joannem (33) Nixonum epiſtolâ, ita ſe habent verba. " Sed cum Monneſes regnum " uſurpaverit, legitimoſque ſpoliaverit hæredes, vi- " cinaque devaſtaverit regna, nil mirum ſi *Victo-* " *riam* in nummis inſculpſerit, atque ad Barba- " ricam, non ad Romanam, referri debet." Hinc enim abundè conſtat, docente nos vel Corſino, vel Venuto, Monneſen monetam ſuam *Victoriâ* ſignâſſe, proptereà quod " regnum uſurpaverat, legi- " timoſque ſpoliaverat hæredes, vicinaque devaſta- " verat regna," quanquam de prælio inter eum et Barbaros commiſſo omninò ſileat hiſtoria. Aſt hoc minimè patitur inſtituti Corſiniani ratio, quandoquidem anteà " dubitaverat de *Victoriâ* in poſticâ " exculptâ" Vir doctiſſimus, " cum eo anno nulla " interfuerit pugna inter Romanos et Barbaros," ſi ex hiſtoricorum ſilentio id evinci poſſit. Pari igitur jure *Victoriam* Barbaricam, ſi hujuſmodi valeat ratio, ac Romanam repudiemus.

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(32) Venut. ubi ſup.
(33) Idem ibid.

X.

X.

Sed ut verum apertè atque ingenuè fateamur, id genus argumenta, ex hiſtoriæ ſilentio ducta, facilè et nullo negotio, advocatâ præſertìm nummorum fide, haud rarò refelluntur. Victorias Romanorum copiis partas præſtantiora interdum memorant numiſmata, ab eorum ſcriptoribus ſilentio penitus prætermiſſas; cujus, ut alia nunc non in medium proferam, nobis exemplo ſit VICTORIA GOTTHI. P. (34) à Taciti imperatoris nummo ante oculos poſita, et cæteroquin (ut opinor) minimùm nota. Parthos igitur Romanorum aciem non profligâſſe, eodem quo Meſopotamiæ urbes ceperunt anno, utcunque de pugnâ taceat hiſtoria, pro certo dicere non auſim. Immò Parthos cum Romanis tùm dimicâſſe à vero non uſquè adeò forſan alienum reperietur, ſi Vaillantium, cujus de Monneſe ſenſa preſſè ſatis ſequitur (35) Cl. Corſinus, perpendamus (36) innuiſſe, " hunc prin-
" cipem ΣΩΤΗΡΟΣ epitheton ſibi adrogaviſſe, ob
" egregium quoddam adverſus Romanos facinus bel-
" licum; quo populorum omnium ſibi benevolen-
" tiam atque amorem conciliavit, quoque in turbu-
" lento illo rerum ſtatu ad Parthicum ſolium viam
" ſibi patefecit." Utut tamen eſt, nil mirum ſi *Victoriam*, more Romano *Gradientem*, Monneſis nummus jam oſtentet; cùm bella (37) contra Romanos feliciter geſſerit, et totam fere Meſopotamiæ provinciam, ſecundum (38) Dionem, perdomuerit.

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(34) Franciſc. Mediobarb. Birag. ubi ſup. p. 410. Mediol. 1683.
(35) Edv. Corſin. apud Venut. ubi ſup.
(36) J. F. Vaill. ubi ſup. p. 340.
(37) Idem ibid.
(38) Dio, Lib. lxxv. p. 853. *Philoſ. Tranſ.* Vol. L. p. 184, 185.

LXVII. *An Account of a ſtony Concretion taken from the Colon of an Horſe; by Mr.* H. Baker, *F. R. S.*

Read May 8, 1760. I Have now the honour to lay before this Royal Society one half of a ſtony concretion, formed in the colon of an horſe, which was lately ſent me from Norwich, by my very curious friend Mr. William Arderon, F. R. S. This horſe, he informs me, was twenty-five years old, had been often ill, and under the farrier's care; which made him deſire to have it opened, when dead.

The whole ſtone, when firſt taken out, weighed ten pounds and half; and he cauſed it to be ſawed in two, that a better judgment might be made of the manner of its formation. Several ſmaller concretions were alſo found in the colon, ſome whereof are ſhewn herewith; but they are much leſs ſolid, and more irregular, than the large one.

This ball is full ſeven inches in diameter, and conſiſts of many laminæ, or coats, which form a number of concentric circles, around a nucleus in the center, which ſeems to be a ſmall ſhiver of black flint. Fifteen or ſixteeen of theſe coats are eaſily diſtinguiſhable, and ſome have been broken off: they vary ſomething in colour, and are, in general, ſo ſtony, that they would probably take a pretty good poliſh. The coats differ in thickneſs, according, perhaps, to the time they were in forming: for it ſhould ſeem, if conjecture may be allowed, that each coat was formed in a longer or ſhorter time, according to

its thickneſs; and that, between the finiſhing of one coat and the beginning of the next, there was ſome interval of time, and ſome ſuſpenſion of that attractive power whereby, or of that component matter whereof, the ſeveral coats are reſpectively formed.

Strand, May 8, 1760. H. Baker.

LXVIII. *An Explanation of the Modes or Tones in the antient* Græcian *Muſic*; *by Sir* Francis Haſkins Eyles Stiles, *Bart. F. R. S.*

Read December 1759, and January, &c. 1760.

NOtwithſtanding the valuable editions of the antient muſical and harmonic writers, given by Meibomius and Wallis, and the great pains thoſe two learned editors have beſtowed on the correcting the tables, and throwing light on the difficult paſſages of thoſe works, there are ſome of the doctrines delivered in them, that ſeem ſtill to require a more perfect explanation. Thoſe, that reſpect the tones or modes, in particular, ſeem to have been ill underſtood: and as it was on theſe, that the effects of the antient muſic were ſuppoſed principally to depend, I have thought, that the ſubject might well merit a re-examination.

Concerning theſe modes, we find two diſtinct, and ſeemingly contradictory doctrines delivered by the antients; and this it is, which has perplexed the ſub-

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ject; for ſome, not aware of the diſtinction, have charged the antients with contradictions; and others, who perceived the two doctrines, not being able to reconcile them, have either adopted one, and rejected the other, or given up the ſubject as hopeleſs: but, as they were both admitted by the antients, they muſt both have been true, in ſome ſenſe. What, therefore, I have principally in view in theſe ſheets, is to ſhew, that the difference between the doctrines aroſe only from the different way of conſidering one and the ſame object; and that therefore there was ſuch an agreement betwixt them, as that, under certain reſtrictions, they may be embraced under one common interpretation.

For diſtinction ſake, I ſhall call one of theſe doctrines the harmonic, and the other the muſical doctrine; the reaſon of which will ſufficiently appear, when I come to treat of the diſtinction between the ſcience of harmonic and that of melopœïa or muſical compoſition.

According to the harmonic doctrine, the number of the modes had been augmented to fifteen; but as Ptolemey, who appears to have favoured the muſical, reduces them to ſeven, and as it is on the principles of that writer, that I propoſe to ſhew an agreement between the doctrines, it will be neceſſary for me, in treating of the harmonic modes, to diſtinguiſh the ſeven he admits from the eight he has excluded.

This being premiſed, I ſhall be underſtood in making the diſtribution of what I have to offer upon this ſubject, which I propoſe to treat under the following ſix heads.

Firſt,

Firſt, To explain the harmonic doctrine, as far as concerns the ſeven modes admitted by Ptolemey.

Secondly, To explain the muſical doctrine of the ſame modes, and ſhew its agreement with the harmonic.

Thirdly, To explain the eight harmonic modes rejected by Ptolemey, and give at large his reaſons for excluding them.

Fourthly, To point out ſomething of the origin of the two doctrines.

Fifthly, To ſhew how far the preceding explanations may be ſupported by arguments, or warranted by the teſtimony of antient writers.

Laſtly, To conſider how this ſubject has been underſtood by Meibomius, Wallis, and ſome few others that have wrote ſince, and in what reſpect their explanations differ from my own.

I now proceed to the firſt head; *viz.*

Firſt, To explain the harmonic doctrine, as far as concerns the ſeven modes admitted by Ptolemey.

The harmonic doctrine of the tones we find expreſly delivered in the harmonic treatiſes; which, probably, is the reaſon, why, among the moderns, the later writers at leaſt have, for the moſt part, determined themſelves in its favour: how juſtly it is intitled to the preference, I ſhall have ſufficient occaſion to examine. The treatiſes on harmonic, that are come down to us, are moſtly Ariſtoxenian, which ſchool ſeems to have treated the ſcience, if not better, at leaſt

leaſt more methodically than any other, of which we can now form a judgment; and hence we find their diviſions often adopted by antient writers, who might not, perhaps, be ready to admit all their principles. In conſidering the phyſical properties of ſound, and the ratios of intervals, the Ariſtoxenians appear to have been leſs exact than the Pythagoreans, the doctrines of the former being more adapted to the groſs and familiar notions of the practical muſician, than to the accurate ſpeculations of the philoſopher. But, however exceptionable their treatiſes may have been in this reſpect, they are the more valuable to us, on this very account, as they give more light into the antient practice of muſic; which is what is chiefly deſired, the philoſophic principles of the ſcience being better underſtood. By this ſchool harmonic was divided into theſe ſeven parts; 1. of ſounds, 2. of intervals, 3. of genera, 4. of ſyſtems, 5. of tones, 6. of mutations, 7. of melopœïa. The propriety of their adding this laſt diviſion I ſhall have occaſion to conſider. Of theſe diviſions, it was the fifth, which contained the doctrine in queſtion; but, to complete it, the ſixth muſt alſo be taken in; for, amongſt other mutations, that of the tones was there treated of, and was indeed the moſt conſiderable object of that diviſion. This doctrine taught, that the difference between one tone and another lay in the tenſion or pitch of the ſyſtem. The ſyſtem (by which I mean the greater perfect one, excluſive of the leſs, of which I ſhall have little occaſion to ſpeak) conſiſted of fifteen ſounds, extending to a diſdiapaſon, or double octave. How theſe ſounds were denominated, and at what intervals they ſucceeded each other, in

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the diatonic genus, to which I ſhall confine this explanation, is ſo well known, that I need ſay no more concerning the ſtructure of the ſyſtem, than that it anſwered to that of our natural ſcale, beginning with Are, and ending with Alamire. This ſyſtem was held immutable, as to the relation of its parts one to another; that is, the order of ſounds and intervals, by which it proceeded, was in the ſame genus to be always the ſame; but the tenſion or pitch of the whole was variable, a different one being aſſigned for each mode. The explanation, therefore, uſually given of the ſyſtem, by comparing it, as I have done, to a double octave, from A in our natural ſcale, is not to be underſtood as fixing its pitch, but as ſhewing its ſucceſſion only; which might as well be done by a double octave from B, taking C# and F# into the ſcale, or from any other note, taking in the flats and ſharps neceſſary to make the tones and ſemitones follow in the ſame order.

The relation of the parts to each other in the ſyſtem being immutable, the fixing the pitch of any one ſound in it, for any mode, was ſufficient to determine that of all the reſt. For this purpoſe, the ſound meſe was commonly uſed; which, by its ſituation, was well adapted to it, being the middle ſound of the ſyſtem. If, therefore, we ſettle the poſition of the meſe for each mode, we ſhall do all, that is requiſite for the clearing up and eſtabliſhing the harmonic doctrine, which conſidered the modes as differing only in the pitch of the ſyſtem.

The modes admitted by the Ariſtoxenians were thirteen in number; to which two more were added by later harmonicians; and to the meſe of each of

theſe fifteen a diſtinct pitch was aſſigned; but, as Ptolemey has rejected eight of them, I ſhall, as I have propoſed firſt, ſeparately conſider the poſitions of the meſe for the ſeven modes he admits.

Concerning the relative pitch of the reſpective meſes for theſe ſeven modes, I find no diſagreement amongſt the harmonic writers. There are not wanting, indeed, who charge the antients with giving, in reſpect thereto, contradictory accounts. Amongſt others, the learned Dr. Gregory aſſerts, that Ariſtides Quintilianus inverts the order of the modes (1): but what led the doctor into this miſtake, was his not diſtinguiſhing the double doctrine. Ariſtides, in the paſſage cited, is not ſpeaking of the pitch of the ſyſtem for the ſeven modes in queſtion, but of the ſeven ſpecies of diapaſon, as they lay in the ſyſtem; which was, indeed, in the inverted order of the meſes of the ſeven modes, as will appear, when we come to conſider the other doctrine. This, then, is no contradiction in the Greek writer, nor, if it were, would it be chargeable ſingly on him; ſince, if the doctor had but caſt his eye on his own Euclid (2), he would have met with the very ſame doctrine he found in Ariſtides. But his remark is, indeed, entirely without foundation; and I make no ſcruple to aſſert, that the antients agree in their accounts of the relative pitch of the meſes, for theſe ſeven modes;

(1) Atque Ariſtides Quintilianus (pag. 18. editionis Meibomianæ) tonum Hypodorium acutiorem facit quam Dorium, et Hypophrygium quam Phrygium, et Hypolydium quam Lydium. Vide Præfat. ad Opera Euclid.

(2) Pag. 540. verſ. 6. et ſeq. ſeu pag. 15. verſ. 15. edit. Meibom.

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for though many of the harmonic writers treat of the thirteen or fifteen modes all together, without diſtinguiſhing the ſeven in queſtion, yet they have given them in ſuch order, and at ſuch diſtances, that the ſeven ſtand at the ſame intervals from each other, as are aſſigned for them by thoſe, who have treated of them ſeparately; ſo that all the accounts agree in this particular. The direction we may conveniently take from Bacchius (3), where it is clearly and conciſely delivered; and where it appears, that the Mixolydian was the moſt acute; the Lydian, graver by a ſemitone; the Phrygian, graver than the Lydian by a tone; the Dorian, graver than the Phrygian by a tone; the Hypolydian, graver than the Dorian by a ſemitone; the Hypophrygian, graver than the Hypolydian by a tone; and the Hypodorian, graver than the Hypophrygian by a tone. Now, as the Guidonian ſcale, in uſe amongſt the moderns, when ſtripped of Guido's additions, anſwers to the ſyſtem of the antients, in its natural ſituation, which was in the Dorian mode, and our Alamire conſequently anſwers to the pitch of the Dorian meſe, we have a plain direction for finding the abſolute pitch of the meſes, for all the ſeven in our modern notes, and they will be found to ſtand thus:

Mixolydian meſe in *d*	Hypolydian in - - *g*#
Lydian in - - - *c*#	Hypophrygian in - *f*#
Phrygian in - - *b*	Hypodorian in - - *e*
Dorian in - - - *a*	

But to underſtand this doctrine, as it is delivered by the antients, it will be neceſſary alſo to examine, how

(3) Pag. 12. edit. Meibom.

the meſes of the ſeven modes were ſtationed upon the lyre; and, in order to this, we muſt conſider the ſtructure of that inſtrument. The lyre, after its laſt enlargement, conſiſted of fifteen ſtrings, which took in the compaſs of a diſdiapaſon, or double octave. Theſe ſtrings were called by the ſame names as the fifteen ſounds of the ſyſtem, and when tuned for the Dorian mode, correſponded exactly with them. Indeed there can be no doubt, but that the theory of the ſyſtem had been originally drawn from the practice of the lyre in this mode, which was the favourite one of the Greeks, as the lyre was alſo their favourite inſtrument. In this mode, then, the meſe of the ſyſtem was placed in the meſe of the lyre; but in every one of the reſt, it was applied to a different ſtring, and every ſound of the ſyſtem tranſpoſed accordingly. Hence aroſe the diſtinction between a ſound in power and a ſound in poſition; for when the ſyſtem was tranſpoſed from the Dorian to any other mode, (ſuppoſe, for inſtance, to the Phrygian) the meſe of the lyre, though ſtill meſe in poſition, acquired, in this caſe, the power of the lichanos meſon; and the parameſe of the lyre, though ſtill parameſe in poſition, acquired the power of the meſe. In theſe tranſpoſitions, one or more of the ſtrings always required new tunings, to preſerve the relations of the ſyſtem; but, notwithſtanding this alteration of their pitch, they retained their old names, when ſpoken of, in reſpect to their poſitions only; for the name implied not any particular pitch of the ſtring, but only its place upon the lyre, in the numerical order, reckoning the proſlambanomenos for the firſt.

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I thought this remark the more neceſſary, as I ſuſpect it was the not attending to this circumſtance, that led Dr. Wallis to aſſign *c* and *g* natural, for the places of the meſe in the Lydian and Hypolydian modes, inſtead of *c*# and *g*#, where I have placed them (4). But, to return,

We are now to give the places of the meſe for theſe ſeven modes upon the lyre, which are thus ſettled by Ptolemey (5).

			String
Mixolydian	Meſe in	Paranetediezeugmenon	11
Lydian		Tritediezeugmenon -	10
Phrygian		Parameſe - - - -	9
Dorian		Meſe - - - - -	8
Hypolydian		Lichanos meſon - -	7
Hypophrygian		Parhypate meſon - -	6
Hypodorian		Hypate meſon - - -	5

The poſitions aſſigned for the meſes of theſe ſeven modes, by the harmonic doctrine, being thus ſettled, both in our modern ſcale, and upon the lyre, I come now,

Secondly, To explain the muſical doctrine of the ſame modes, and ſhew its agreement with the harmonic.

The muſical doctrine taught, that the difference between one mode and another conſiſted in the manner of dividing an octave, or, as the antients expreſs it, in the different ſpecies of diapaſon. The elementary principles of theſe ſpecies we find delivered

(4) See his edition of Ptolemey, p. 137.
(5) Ibidem.

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in the harmonic writers; but as in the Ariſtoxenian treatiſes, they are not found under the fifth diviſion, of tones; nor under the ſixth, of mutations, where it was natural to expect them; but under the fourth, of ſyſtems; and it not being there expreſly affirmed, that the ſpecies had a relation to the tones, though, from their denominations, and other circumſtances, it might well be inferred, this has created a difficulty in admitting their connection with the ſubject. The obviating of this objection I ſhall reſerve for my fifth head; where I ſhall take occaſion, particularly, to juſtify this doctrine, and the uſe I ſhall have made of it; and I ſhall therefore proceed to explain it, as I find it in the harmonic writers.

To underſtand how this doctrine of the ſpecies of diapaſon came to be treated under the head of ſyſtems, it will be neceſſary to conſider the definition given of the term ſyſtem by the Ariſtoxenians, and their ſubdiviſion of this branch of harmonic.

With this ſchool, whatever conſiſted of more than one interval, was a ſyſtem. So comprehenſive a definition could not but make this branch a very large one; and ſo we accordingly find it. It was ſubdivided in the manner following: ſyſtems were there conſidered as differing in reſpect, firſt, to magnitude; ſecondly, to genus; thirdly, to the being conſonant or diſſonant; fourthly, to the being rational or irrational; fifthly, to the being ſequent or tranſgreſſive; ſixthly, to the being conjunct or disjunct; ſeventhly, to the being mutable or immutable. Now, of theſe ſeven differences, it was under the third that the doctrine of the ſpecies of diapaſon came to be treated, the conſonant ſyſtems being there enumerated, and explained.

explained. By a conſonant ſyſtem was underſtood a ſyſtem, whoſe extreme or comprehending ſounds were conſonant: of theſe, there were ſix within the compaſs of the immutable ſyſtem, *viz.* the diateſſaron, diapente, diapaſon, diapaſon and diateſſaron, diapaſon and diapente, and diſdiapaſon. Theſe conſonant ſyſtems were conſidered as admitting each of a variety in the order of the intervals, of which it was compoſed. A ſyſtem of a determined magnitude, compoſed of the ſame intervals, and of the ſame number of them, might, if the component intervals were not equal, differ in reſpect to their order or ſucceſſion. Theſe variations of each ſyſtem they called εἶδος, its ſpecies, or σχῆμα, its figure, for theſe terms were ſynonymous (6). The ſpecies of diateſſaron were three; thoſe of the diapente four, and thoſe of the diapaſon ſeven, being the ſum of the ſpecies of the other two conſonances, of which the diapaſon itſelf was compoſed: and here the doctrine of the ſpecies ſtopped; for, in the three larger conſonant ſyſtems, they ſeem not to have conſidered the ſpecies of the whole magnitude, but only thoſe of the three ſmaller ſyſtems, of which they were compoſed, and which are called, by Ptolemey, the firſt conſonances (7). As the ſpecies of diateſſaron and diapente do not immediately concern this doctrine, the explaining them will be unneceſſary; and I ſhall therefore proceed to thoſe of the diapaſon.

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(6) Μετὰ δὲ ταῦτα λεκτέον, Τίς ἐστι, καὶ πόια τις, ἢ κατ' εἶδος διαφορά; διαφέρει δ' ἡμῖν εἰδέναι, εἶδος λέγειν ἢ σχῆμα· φέρομεν γὰρ ἀμφότερα τὰ ὀνόματα ταῦτα ἐπὶ τὸ αὐτό. Ariſtox. p. 74. v. 9.

(7) Πρῶτον δεικτέον τὰς κατὰ τὸ καλούμενον εἶδος τῶν πρώτων συμφωνιῶν διαφοράς. Ptol. lib. ii. c. 3.

Concerning

Concerning theſe, I find no diſagreement amongſt the writers of antiquity: they all agree, that they were ſeven in number, and had the denominations of Mixolydian, Lydian, Phrygian, Dorian, Hypolydian, Hypophrygian, and Hypodorian; circumſtances, which leave no doubt of their connection with the modes. The ſucceſſion of intervals, or manner of dividing the diapaſon for each ſpecies, has been variouſly explained by the antient writers; but the reſult of theſe explanations is the ſame, excepting the generic differences. It will be ſufficient here to give what reſpects the diatonic genus only, as the queſtion concerning the tones does not turn upon the diſtinction of the genera.

In the diatonic genus, the diapaſon conſiſting of five tonic and two ſemitonic intervals, the Ariſtoxenians fixed the ſucceſſion for each ſpecies, by the poſition of the two ſemitones; ſhewing, at the ſame time, between which of the ſounds of the immutable ſyſtem the ſpecies in queſtion was comprehended: for this ſyſtem conſiſting of fifteen ſounds, contained eight octaves, two of which, *viz.* that from proſlambanomenos to meſe, and that from meſe again to netehyperboleon, were divided after the ſame manner, and therefore conſtituted the ſame ſpecies: but the ſix others were all differently divided; ſo that the ſyſtem contained within itſelf the ſeven ſpecies of diapaſon, and thence obtained the denomination of perfect; the leſſer perfect ſyſtem, which reached only to a diapaſon and diateſſaron, being improperly ſo called for want of this qualification, as Ptolemey has

has ſhewn (8). According to this method of explaining the ſpecies of diapaſon, they ſtand as follows:

Species.	
1. Mixolydian	From hypate hypaton to parameſe. Semitones, firſt interval in the grave, fourth in the acute.
2. Lydian	From parhypate hypaton to trite diezeugmenon. Semitones, third interval in the grave, firſt in the acute.
3. Phrygian	From lichanos meſon to paranete diezeugmenon. Semitones, ſecond interval in the grave, ſecond in the acute.
4. Dorian	From hypate meſon to nete diezeugmenon. Semitones, firſt interval in the grave, third in the acute.
5. Hypolydian	From parhypate meſon to trite hyperbolæon. Semitones, fourth interval in the grave, firſt in the acute.
6. Hypophrygian	From lichanos meſon to paranete hyperbolæon. Semitones, third interval in the grave, ſecond in the acute.

(8) Ὅθεν τὸ συντιθέμενον ἐκ τῶ διαπασῶν ϗ διὰ τεσσάρων σύστημα τέλειον ὡ καλῶς ἔχει καλεῖν· τὰ μὲν γὰρ ἐπὶ ἐλάττω τῶ διαπασῶν ἀδύνατον τελειέξει. Ptol. Harm. p. 106. v. 3.

Species.	
7. Hypodorian	From meſe to nete hyperbolæon, or proſl. to meſe. Semitones, ſecond interval in the grave, third in the acute.

It muſt be here obſerved, that theſe ſpecies, as they ſtand in the ſyſtem, are, with reſpect to acuteneſs and gravity, in the inverted order of the ſeven modes, as ſettled by the harmonic doctrine, which will account for the miſtake, which Dr. Gregory (as was ſhewn above) and others have fallen into, in imagining the antients did not agree in their accounts of the modes; ſince it is plain theſe writers did not obſerve, that, in the one caſe, the modes were ſpoken of with reſpect to the poſition of the ſyſtem itſelf, and in the other, with reſpect to the poſition of the ſeven ſpecies within the ſyſtem.

Theſe ſpecies may alſo be more readily ſhewn, by examples out of the Guidonian ſcale, wh. e, keeping the natural notes, they will ſtand thus:

Mixolydian	from	B to *b*.
Lydian		C to *c*.
Phrygian		D to *d*.
Dorian		E to *e*.
Hypolydian		F to *f*.
Hypophrygian		G to *g*.
Hypodorian		*e* to *aa*, or A to *a*.

But as well this, as the former way of ſhewing them, we are to underſtand only as exemplifications of the ſucceſſion of the intervals, and not as an aſſignment of the pitch for each, which was to depend on that of the ſyſtem, out of which they are exemplified;

and

and it may not be amiſs therefore, in order to prevent any limited conception of theſe ſpecies, to ſtate generally the order of the intervals, of which each is compoſed; which will be as follows, proceeding in each from grave to acute.

Mixolydian.	Semitone, tone, tone, ſemitone, tone, tone, tone.
Lydian.	Tone, tone, ſemitone, tone, tone, tone, ſemitone.
Phrygian.	Tone, ſemitone, tone, tone, tone, ſemitone, tone.
Dorian.	Semitone, tone, tone, tone, ſemitone, tone, tone.
Hypolydian.	Tone, tone, tone, ſemitone, tone, tone, ſemitone.
Hypophrygian.	Tone, tone, ſemitone, tone, tone, ſemitone, tone.
Hypodorian.	Tone, ſemitone, tone, tone, ſemitone, tone, tone.

Beſides the manner above-mentioned of explaining the ſpecies of diapaſon, the antients have given us another, the reſult of which is the ſame; and that is, by the poſition of the diazeuctic tone, or interval from meſe to parameſe. In the Mixolydian ſpecies, the diazeuctic tone was the firſt interval, reckoning from acute to grave; in the Lydian, it was the ſecond; in the Phrygian, the third; in the Dorian, the fourth; in the Hypolydian, the fifth; in the Hypophrygian, the ſixth; and in the Hypodorian, the laſt.

Now, either of theſe methods fixes the ſucceſſion of intervals peculiar to each ſpecies; but as the examples are taken from a ſyſtem, whoſe pitch was variable, we are ſtill to ſeek, at what abſolute pitch the ſeveral ſpecies were taken in the modes, to which we ſuppoſe them to have been reſpectively ſubſervient; and it is, perhaps, the ſeeming difficulty of ſettling this, that has induced ſo many to reject this doctrine entirely, and fall in with the harmonic writers, who conſidered the modes as differing only

in the pitch of the ſyſtem: but, by taking in the aſſiſtance of the harmonic doctrine, we ſhall not only gain the true pitch for each ſpecies in the muſical, but be led to ſee the agreement between the two doctrines, which, I have already ſaid, was my principal view in theſe ſheets, and which, having gone through the explanation of each doctrine ſeparately, as far as concerns the ſeven modes, we are now ripe for conſidering; for the eight modes, whoſe explanation I have deferred, have no concern in this agreement, being rejected by Ptolemey, as will appear, for this very reaſon, in ſubſtance, that they would ſtand the trial of the harmonic doctrine only, whereas the reſt had the ſupport of both. Let us then conſider how the two doctrines, as I have explained them, may be made to agree.

By the harmonic doctrine, we are told the pitch of the ſyſtem for each mode; and by the muſical, in what part of the ſyſtem to take the ſpecies of diapaſon: now, by combining theſe two directions, we gain the following plain canon, for finding any mode required.

Canon.

First pitch the ſyſtem for the mode, as directed by the harmonic doctrine; then ſelect from it the diapaſon, directed by the muſical; and we have the characteriſtic ſpecies of the mode in its true pitch.

To make this more plainly appear, and alſo to avoid the length of particular explanations, I have annexed a diagram of the ſeven ſpecies, which will ſhew at what pitch of the Guidonian ſcale each ſound of

d by PTOLEMEY.

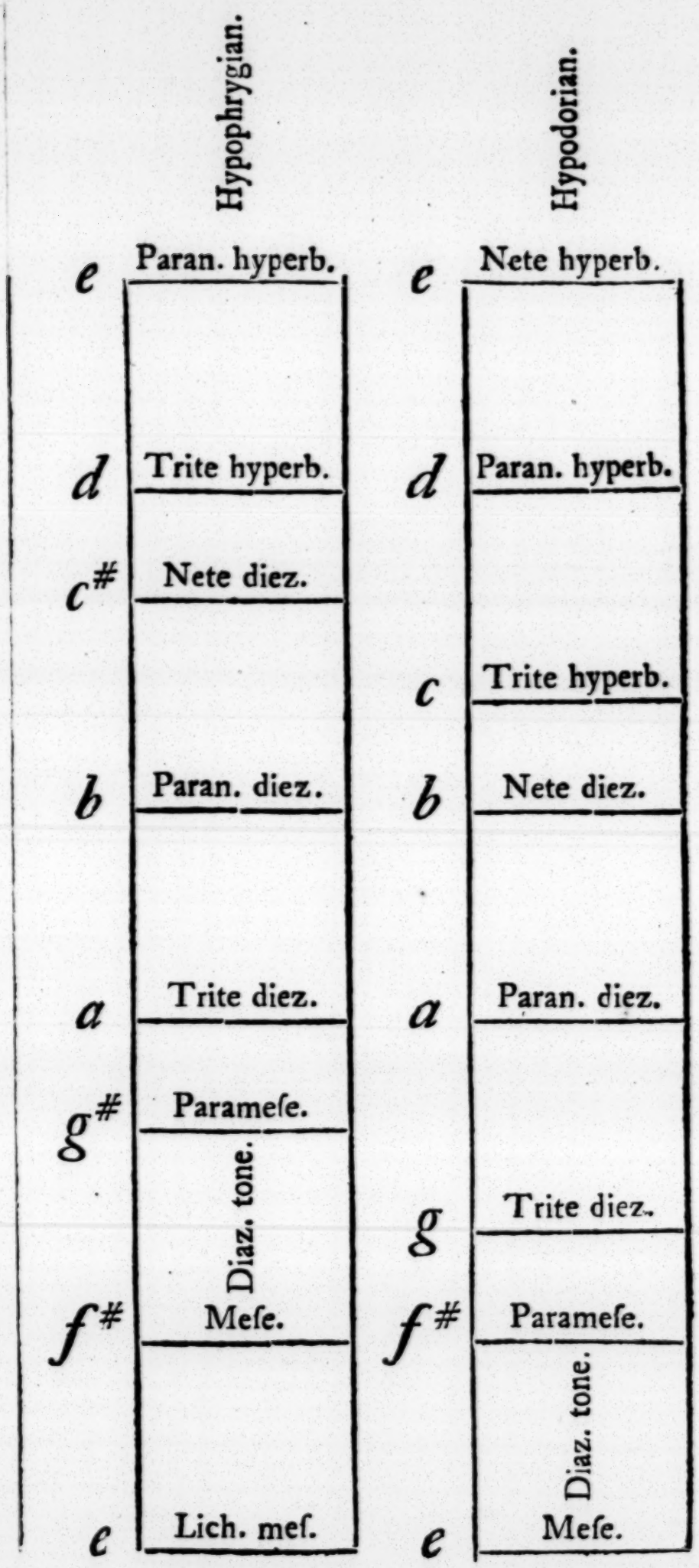
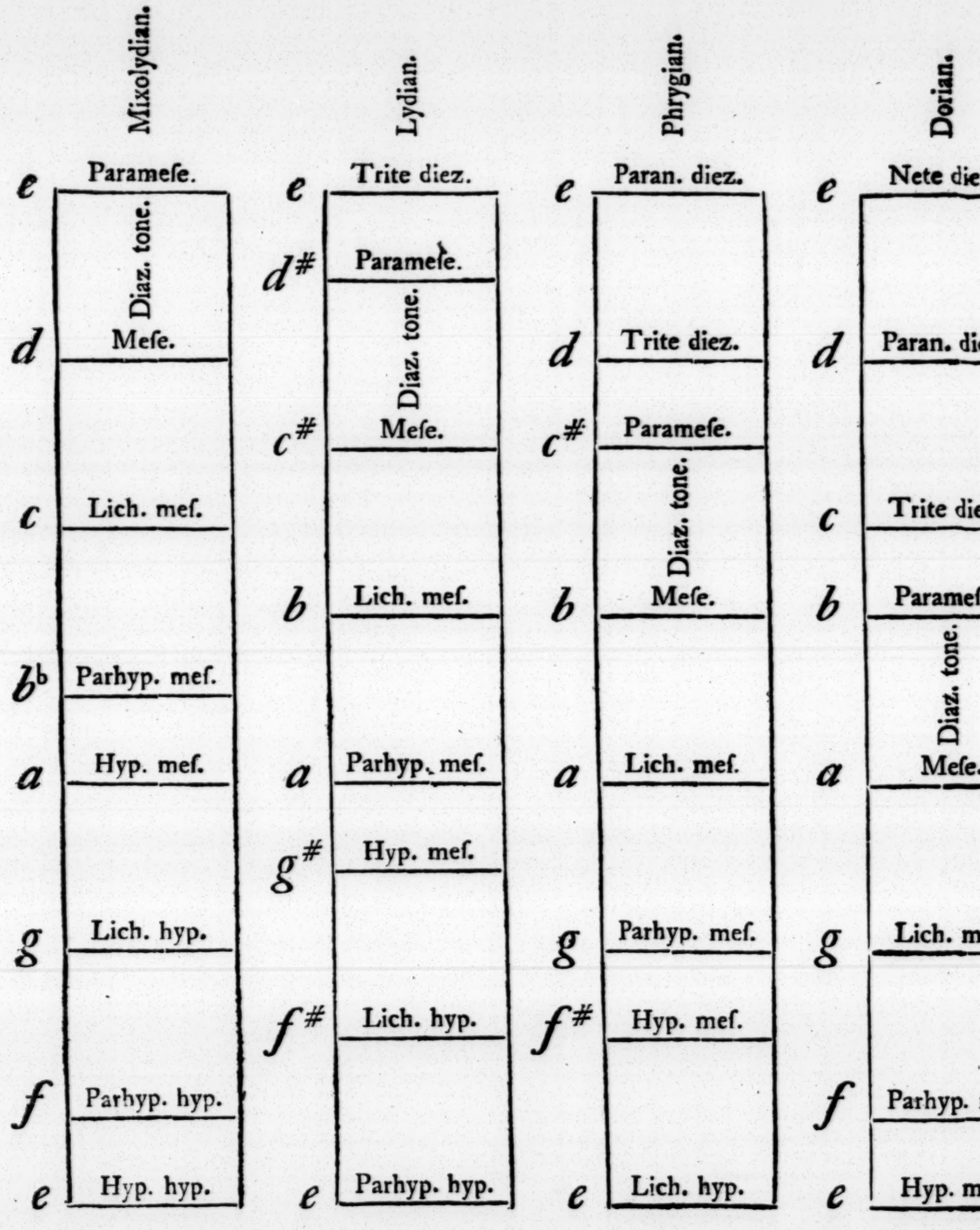


DIAGRAM I. Of the Species of Diapaſon in the



on in the Seven Modes admitted by PTOLEMEY.

Dorian.		Hypolydian.	Hypophrygian.		Hypodorian.	
Nete diez.	*e*	Trite hyperb.	*e*	Paran. hyperb.	*e*	Nete hyperb.
	d#	Nete diez.				
Paran. diez.			*d*	Trite hyperb.	*d*	Paran. hyperb.
	c#	Paran. diez.	*c*#	Nete diez.		
Trite diez.					*c*	Trite hyperb.
Parameſe.	*b*	Trite diez.	*b*	Paran. diez.	*b*	Nete diez.
Diaz. tone.						
Meſe.	*a*#	Parameſe.	*a*	Trite diez.	*a*	Paran. diez.
	g#	Diaz. tone. Meſe.	*g*#	Parameſe.		
Lich. meſ.				Diaz. tone.	*g*	Trite diez.
	f#	Lich. meſ.	*f*#	Meſe.	*f*#	Parameſe.
Parhyp. meſ.						Diaz. tone.
Hyp. meſ.	*e*	Parhyp. meſ.	*e*	Lich. meſ.	*e*	Meſe.

of the diapaſon is brought out by the canon for each of the ſeven modes. As in the conſtruction of this diagram, the directions of the canon have been ſtrictly purſued, ſo it will appear, that the reſult of it is, in all reſpects, conformable to the principles of both doctrines. Thus in the Dorian, for inſtance, it will be ſeen, that the meſe is placed in Alamire, and that the reſt of the ſounds exhibited in that diapaſon, are placed at the proper diſtances, for preſerving the order of the ſyſtem, as required by the harmonic doctrine. It will be alſo ſeen, that the diapaſon ſelected lies between hypate meſon and nete diezeugmenon; that the ſemitones are the firſt interval in the grave, and third in the acute; and that the diazeuctic tone is in the fourth interval, reckoning from the acute; all which circumſtances were alſo required by the muſical doctrine for this mode; and, in the reſt of the modes, all the circumſtances required by each doctrine will, in like manner, be found to obtain: ſo that no objection can well be raiſed to the principles, on which the diagram has been framed, by the favourers of either doctrine ſeparately; and the very coincidence of the two doctrines therein, might furniſh a probable argument in juſtification of the manner, in which I have combined them in the canon. But as I propoſe to conſider this under the fifth head, where the proofs will be collected, I ſhall leave this argument for the preſent, and proceed to explain,

Thirdly, The eight harmonic modes rejected by Ptolemey, and give at large his reaſons for excluding them.

Six-

Six of theſe modes, with the ſeven Ptolemey allowed, made up the thirteen allowed by the Ariſtoxenians: the other two ſeem to have been added afterwards, more with a view to regularity in the names and poſitions of the modes, than to any particular uſe, as will appear in the courſe of the explanation. For the ſettling theſe eight modes, I ſhall have no occaſion to go farther than the fixing the poſition of the meſe for each, according to the harmonic doctrine: the reſult of applying the muſical doctrine to them will ſufficiently appear, when I come to explain the reaſons, which Ptolemey has aſſigned for rejecting tnem.

The old Dorian, Phrygian, and Lydian modes, having, as we have ſeen, their meſes reſpectively in *a*, *b*, *c*#, at the diſtance of a tone from each other, theſe tonic ſpaces were afterwards divided, to make room for the Ionian meſe in *b* flat, and the Æolian in *c* natural. To theſe five modes, whoſe meſes ſucceeded each other at the interval of a ſemitone, and which came, in time, to be called the middle modes, five others were made to correſpond, that lay reſpectively graver by a fourth, and took their denominations from them, but compounded with the prepoſition *hypo*, to diſtinguiſh them. Of theſe, we have already ſhewn the Hypodorian meſe to have been in *e*, the Hypophrygian in *f*#, and the Hypolydian in *g*#. Now, the two tonic ſpaces between *e*, *f*#, and *g*#, being likewiſe diviſible, the Hypoionian meſe was inſerted in *f* natural, and the Hypoæolian in *g* natural, at a fourth reſpectively from the Ionian and Æolian. To theſe ten modes, it was conceived that five more might properly be ranged towards

towards the acute, at the diſtances of a fourth reſpectively from the five firſt mentioned, and taking their denominations from them alſo, but compounded with the prepoſition *hyper*, which would complete them to fifteen, divided into five grave, five middle, and five acute. But the Ariſtoxenians limiting the poſitions of the meſe to the compaſs of an octave, had eſtabliſhed only three out of theſe laſt five, *viz.* the old Mixolydian, whoſe meſe we have ſhewn to be in *d*, at a fourth from the Dorian, and which was therefore afterwards called Hyperdorian; the Hyperionian, whoſe meſe was in *e* flat, at a fourth from the Ionian; and the Hypermixolydian (for ſo it had been originally named, the prepoſition being taken in a different ſenſe), the meſe of which was in *e*, at a fourth from the Phrygian, and which was therefore afterwards called the Hyperphrygian. To make theſe acute modes therefore five in number, as well as the grave and middle ones, two new modes were added, *viz.* the Hyperæolian, whoſe meſe was in *f*, and the Hyperlydian, whoſe meſe was in *f*♯, at a fourth reſpectively from the Æolian and Lydian.

The modes being thus augmented to fifteen, upon the ſingle principle of the harmonic doctrine, their meſes will be found to ſtand all together in the following order.

Acute	Hyperlydian - - - - - - -	in *f*♯
	Hyperæolian - - - - - - - -	*f*
	Hyperphrygian, or Hypermixolydian -	*e*
	Hyperionian - - - - - - - -	*e*♭
	Hyperdorian, or Mixolydian - - - -	*d*

Middle

Middle	Lydian	in	*c*#
	Æolean		*c*
	Phrygian		*b*
	Ionian		*b*♭
	Dorian		*a*
Grave	Hypolydian	in	*g*#
	Hypoæolian		*g*
	Hypophrygian		*f*#
	Hypoionian		*f*
	Hypodorian		*e*

Having now ſhewn, how theſe fifteen modes were ſituated according to the harmonic doctrine, I come to the objections raiſed by Ptolemey againſt the eight laſt explained. For the right underſtanding of which, it will be neceſſary to give an account of all that he has delivered on the ſubject of the modes, a taſk of ſome difficulty; for though his reaſoning appears to me moſt clear and methodical, it is rather too prolix to be given exactly in his own words, as this, with the neceſſary explanations of thoſe paſſages, of which the ſenſe is leſs obvious, would carry me to too great a length. I muſt therefore endeavour to abridge his doctrines, though at the hazard of leaving out ſome of the ſubſtantial parts, and thereby doing injuſtice to his argument.

It will be expedient to premiſe ſomething concerning his apparent intention, and method of reaſoning in this part of his work, which will ſerve as a key to his argument: and this ſeems the more neceſſary, as he has been thought by ſome (9) to

(9) So obſcurely has the beſt of all the antient writers (Ptolemey) delivered himſelf on this article (the tones), that deſerved to have been moſt clearly handled. Malcolm's Treatiſe on Muſic, p. 539.

have

have written very obſcurely upon the ſubject. His chief view was to reduce the fifteen modes, admitted upon the principle of the harmonic doctrine, to thoſe ſeven, which had the ſupport alſo of the muſical: but this reformation was to be attempted with great precaution, on account of the obſtinate prejudices it was likely to meet with, from the harmonicians of his own time: And we ſhall therefore find him firſt artfully treating the modes upon the foot of the harmonic doctrine only, and arguing *ad homines* for the reduction of the modes, even upon their own principles; but, before he drops the ſubject, his true reaſons will appear; and, indeed, it will eaſily be ſeen, that he had them conſtantly in view from the firſt, though he does not argue openly upon them. This will account for that mixture of the two doctrines, which is found in his writings, and will, with the aſſiſtance of a few explanations, render intelligible what he has delivered upon the ſubject.

The tones he conſiders, after the harmonic doctrine, as mutations by whole conſtitutions, which, he tells us, are therefore properly called tones, as they are differences in reſpect to tenſion. Theſe differences he admits to be infinite in poſſibility, but argues, that in efficacy, and in reſpect to ſenſe, they are finite, and liable to certain limitations. Theſe limitations he derives from the theory of the conſonances, by which means, he lays a foundation for his future argument, for reducing the modes to the number of ſpecies of the conſonance diapaſon. The limitations of the tones, which he propoſes to regulate by thoſe of the conſonances, are, as he tells us, threefold; *viz.*

1. As to the diſtance, or ratio, as he expreſſes it, of the extreme tones; for the Pythagoreans, whoſe method he adopts with ſome improvements of his own, meaſured intervals by the ratios of the comprehending ſounds.

2. As to the number of tones to be admitted between theſe extremes. And,

3. As to the intervals, at which they were to ſtand in ſucceſſion, which he calls their exceſſes.

Thus in the diateſſaron conſonance, which he inſtances, theſe three circumſtances are obſervable; firſt, That the ratio of the extreme ſounds is ſeſquitertian; ſecondly, That the component intervals, or ratios, are three; and thirdly, That ſuch and ſuch are the differences of thoſe ratios, meaning the intervals in ſucceſſion. But here he obſerves, that, in the conſonances, theſe limitations have each their diſtinct cauſe; whereas in the tones, the firſt being determined, the other two neceſſarily followed, as being dependent on the ſame conditions. This remark will hardly be intelligible, without ſome explanation. The interval, or ratio of the extreme ſounds in each conſonance, though differently treated by the Ariſtoxenian and Pythagorean ſchools, were yet determined, both by the one and the other, upon principles, which concerned not their intermediate diviſion: their intermediate diviſion again was ſettled by a doctrine, that required, in the compoſition of intervals, either that every fourth ſound ſhould complete the diateſſaron, or every fifth the diapente; without one or other of which circumſtances, the compoſition was held inconcinnous,

nous (10); and hence the diateſſaron could not be divided into more than three ratios, nor the diapente into more than four; and again, the exceſſes of the ratios, or intervals in ſucceſſion, were aſſigned by the doctrine of the genera; ſo that in the conſonances, each of the three limitations had a ſeparate cauſe; but in applying the ſame limitations to the tones, there was a connection between them; ſo that the determining the firſt, determined the reſt. Thus, for inſtance, if the interval diateſſaron, or ſequitertian ratio, ſhould be aſſigned for the limitation of the extreme tones, the other limitations could only be ſuch, as were aſſigned for that conſonance; that is, the component ratios muſt be three, and the exceſſes of thoſe ratios ſuch, as were eſtabliſhed for the diateſſaron by the doctrine of the genera. But the not knowing the neceſſity of this conſequence, he tells us, had occaſioned a great diſagreement amongſt the harmonicians in determining theſe points, ſome making the interval of the extreme tones leſs than a diapaſon, others equal to it, and others again exceeding it; the later harmonicians, in particular, being fond of adding ſomething to what had been admitted by thoſe, who went before them. To bring theſe various opinions to ſome determination, he propoſes the reſtitution of ſimilitude in the hermoſmenon, or harmoniſed melos, as the chief circumſtance, by which to regulate the

(10) Ἐν παντὶ δὲ γένει ἀπὸ παντὸς φθόγγου διὰ τῶν ἑξῆς τὸ μέλος ἀγόμενον, ἢ ἐπὶ τὸ βαρὺ, ἢ ἐπὶ τὸ ὀξύ, ἢ τὸν τέταρτον τὸ ἑξῆς διατεταάρων, ἢ τὸν πέμπτον διαπεντε σύμφωνον λαμβάνεσθαι. ᾧ δ' ἂν μεδετέρα τῶτων συμβαίνῃ, ἐκμελὴς ἔσω ἅπος πρὸς ἅπαντας, ἐν οἷς συμβεβηκεν ἀσυμφώνῳ εἶναι κατὰ τὰς εἰρημένας διαιρέσεις. Ariſtox. p. 54. edit. Meibom.

firſt limitation; and this, he ſays, is effected by the diapaſon, for, at that diſtance, there was always a return of ſimilar harmony; ſo that the tone, that was a diapaſon diſtant from that firſt taken, was a repetition of it; and the tone ſtill farther diſtant, as for inſtance, at a diapaſon and diateſſaron, the ſame with the tone diſtant a diateſſaron only, and ſo of the reſt. For a proof of this, he appeals to the practice of muſic; where it was well known, that in ſuch mutations, as were at the interval of a diapaſon, no one of the ſtrings required new tuning, though in all other mutations ſome alterations were neceſſary. And hence he concludes, that thoſe, who make the diſtance of the extreme tones leſs than the diapaſon, do not reſtitute the hermoſmenon, there being ſtill other tones beyond the limit they aſſign, unlike to thoſe before taken; and that thoſe, who exceed the diapaſon, admit redundancies, by repeating ſome of the harmonies before taken; and further inſiſts, that even they, who proceeded no farther than the diapaſon, were yet to blame, inaſmuch as they took in the tone, that was a diapaſon diſtant from the firſt, ſince, in ſo doing, they admitted one redundancy, and thereby gave a handle to thoſe, whom they charged with paſſing the proper limits for the modes, to accuſe them, in turn, with being really the authors of this licence, ſince, if one uſeleſs tone be admitted, the ſame privilege may be fairly claimed for a greater number. This laſt argument ſeems levelled at the Ariſtoxenians, who admitted no more than thirteen modes; becauſe they would not exceed the diapaſon; but whoſe Hyperphrygian was, according to this argument, a repetition of the Hypodorian. The

author

author ſupports this argument ſtill farther, by an appeal to the circumſtance of the ſpecies of diapaſon, the ſounds of which were eight, but the number of the ſpecies ſeven only, anſwering not to the number of terms in the diviſion, but to that of the component ratios: for that the diapaſon taken from the graveſt ſound towards the grave, yielded the ſame ſpecies with the firſt diapaſon taken from the acuteſt ſound towards the ſame parts, was out of diſpute, it holding true univerſally, that whatever takes its beginning in the ſame manner from either of the extremes of the diapaſon, produces the ſame power. And here he leaves the firſt limitation, without expreſly aſſigning the interval for the extreme tones; for the title of the chapter, which ſeems to fix it to a diapaſon, ought to be underſtood only in this ſenſe, that it ſhould not exceed it; which agrees with the reaſoning in the chapter itſelf. As to the concluſion, which depended on the two other limitations, if I may venture to draw it for him, it will ſtand thus, that into what number ſoever of terms the diapaſon be divided, the diſtance for the extreme tones ſhould be the interval between the firſt term and the laſt but one.

The firſt limitation being thus far conſidered, he proceeds to determine the next, upon which depended the number of the tones; and here he again oppoſes the Ariſtoxenians, rejecting, by his theory of this limitation, five of their thirteen modes, beſides the Hyperphrygian, which ſtood condemned by the former one, and leaving only ſeven, according to the number of the ſpecies of diapaſon, which he propoſes as the propereſt rule, by which to govern this limitation; and aſſigns for this the following reaſons.

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The number of the ſpecies of diapaſon was equal to that of the ſpecies of the two firſt conſonances taken together, and the ſpecies of theſe laſt were taken according to the condition of the ratios in each, the number of which the very nature of them would not permit to be either increaſed or diminiſhed. Now the tones contained within the diapaſon following the nature of the conſonances, and being indeed eſtabliſhed on their account, *viz.* that the whole ſyſtems might have conſonant differences, he argues, that thoſe, who were either for admitting more than ſeven, which was the number both of the ſpecies and of the ratios in the diapaſon, or for making all the exceſſes of the tones equal, were not to be aſſented to, ſince they could not aſſign any ſatisfactory reaſon, either for the equality of the increments in general, which, in the harmonic genus, was particularly inconvenient, or for fixing either on the tone, hemitone, or dieſis, in particular, for the common exceſs, (from the ſuppoſition of one or other of which, they determined the number of tones, according to the number of ſuch intervals contained within the diapaſon). For what was there to determine ſuch a preference, when the conſonance (meaning the diapaſon) was, as they themſelves allowed, ſuſceptible not only of all theſe exceſſes, but of many others, in the orders both of the genera and of the diſtances? Nor could they ſay, that ſuch a magnitude divides the diapaſon exactly, and ſuch another not exactly, or one, perhaps, into an even number of parts, and another into an uneven: for though the diapaſon was divided into ſix by the tone, into twelve by the hemitone, into eighteen by the third of the tone, and into twenty-four by the quarter,

quarter; yet, in all theſe diviſions, there was a variation from the exact truth, even perceptible to the ear. All this reaſoning is again manifeſtly levelled at the Ariſtoxenians, who, falſely eſteeming the diapaſon to conſiſt exactly of ſix tones, or twelve hemitones, had admitted thirteen modes, at the diſtance of a ſemitone from each other: and here we ſee the author ſtill combats them fairly on the principles of the harmonic doctrine. But, to go on with his arguments; having thus fixed the number of the modes to ſeven only, he proceeds to conſider the third limitation. It will be aſked, ſays he, what are to be the exceſſes choſen for theſe ſeven modes, ſince the diapaſon cannot be divided into ſeven equal parts; and, if unequal intervals be admitted, there is ſtill room for variety in the choice? In anſwer to this, he propoſes, that the intervals in queſtion ſhall be taken by means of the firſt conſonances, diateſſaron and diapente, and their differences or exceſſes, whatſoever they may be: for he argues, that the mutations, by conſonant intervals, ought to be firſt eſtabliſhed; and next to theſe, the mutations, by concinnous intervals. Thoſe, who admitted eight tones, he tells us, had found their poſitions after the following manner. The Dorian, Phrygian, and Lydian, the three moſt antient, being firſt ſet at the diſtance of a tone from each other, they made the next mutation conſonant, at a diateſſaron from the Dorian, towards the acute, and at a limma from the Lydian, calling it Mixolydian; becauſe it lay not at the diſtance of a tone from the Lydian, but only at the diſtance of a limma, or difference between ditonus and diateſſaron; then this new mode having

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the Dorian graver than it by a diateſſaron, that the reſt of the four might be attended with the like circumſtance, they eſtabliſhed the Hypolydian, Hypophrygian, and Hypodorian, a diateſſaron graver reſpectively than the Lydian, Phrygian, and Dorian; and laſtly, they placed the Hypermixolydian at a diapaſon from the Hypodorian, towards the acute, giving it that denomination from its poſition above the Mixolydian, miſuſing, as he tells us, the prepoſition *hypo* to ſignify the grave, and *hyper* to expreſs the acute. But though the reſult of this method is the ſame with that of his own, excepting as to the Hypermixolydian, (which he excludes for the reaſon aſſigned in treating of the firſt limitation) yet he objects to it; becauſe the concinnous intervals are firſt aſſumed, which ought to be taken by the conſonances; and gives his own, which is by the continual addition of the conſonance diateſſaron, but, however, within the compaſs of the diapaſon. Wherefore, when ſuch addition would exceed it, he directs taking the diapente in its ſtead, the contrary way; whereby the diviſion will be kept within the limits aſſigned. This method, and its reſult, will perhaps be better underſtood, and will alſo be more ſuccinctly couched in our modern expreſſions, in which I ſhall therefore give it, applying to it the correſponding characters of the Guidonian ſcale.

Having firſt taken the Mixolydian tone, which was the acuteſt of the ſeven, ſuppoſe in D, fall a fourth to A, for the Dorian; another fourth to E, for the Hypodorian; then, to avoid paſſing the bounds of the diapaſon, riſe a fifth to B, for the Phrygian; fall a fourth again to *f*#, for the Hypophrygian;

phrygian; riſe a fifth to *c*#, for the Lydian; and fall a fourth to G#, for the Hypolydian.

By this method, the poſitions of the ſeven modes come out exactly, as I fixed them from Bacchius, in explaining the harmonic doctrine; and we ſee, that, for ſettling them, Ptolemey has really recourſe to no other theory of the modes, than that admitted by thoſe he contends with, though he makes the ſpecies of the conſonances, and thoſe of the diapaſon more particularly, the governing rule for fixing their poſitions, as the only means, by which the two doctrines could be made to coincide. But it remained, after thus ſettling the ſeven modes, to ſhew more fully the conſequence of following the method of the Ariſtoxenians, and others, who divided the tonic ſpaces found by his method, and placed the modes in a ſemitonic ſucceſſion, by which their number had been raiſed to thirteen, even within the compaſs of the diapaſon; and, in doing this, we ſhall find he ventures to aſſign the true reaſon for his reduction, which was grounded on the muſical doctrine. This argument, which is contained in the eleventh chapter of his ſecond book, being very remarkable, and ſeeming ſtrongly to ſupport the combination of the two doctrines in the diagram I have given of the ſeven modes, I ſhall give a tranſlation of the whole chapter, leſt I ſhould be thought to ſtrain his arguments in favour of the muſical doctrine, which has been thought by many to have little or no relation to the modes, and which, if we except what this author has delivered, ſeems indeed, upon a ſlight examination, and compariſon of the evidence, to have the weaker ſupport.

Now, theſe being the tones we have eſtabliſhed, it is plain, that a certain ſound of the diapaſon is appropriated to the meſe, in power of each, by reaſon of their being equal in number to the ſpecies. For a diapaſon being ſelected out of the middle parts of the perfect ſyſtem; that is, the parts from hypate meſon in poſition to nete diezeugmenon, (becauſe the voice is moſt pleaſed to be exerciſed about the middle melodies, ſeldom running to the extremes, becauſe of the difficulty and conſtraint in immoderate intentions and remiſſions), the meſe in power of the Mixolydian will be fitted to the place of paranete diezeugmenon, that the tone may, in this diapaſon, make the firſt ſpecies; that of the Lydian, to the place of trite diezeugmenon, according to the ſecond ſpecies; that of the Phrygian, to the place of parameſe, according to the third ſpecies; that of the Dorian, to the place of the meſe, making the fourth and middle ſpaces of the diapaſon; that of the Hypolydian, to the place of lichanos meſon, according to the fifth ſpecies; that of the Hypophrygian, to the place of parhypate meſon, according to the ſixth ſpecies; that of the Hypodorian, to the place of hypate meſon, according to the ſeventh ſpecies; that ſo it may be poſſible, in the alterations required for the tones, to keep ſome of the ſounds of the ſyſtem unmoved, for preſerving the magnitude of the voice (meaning the pitch of the diapaſon), it being impoſſible for the ſame powers, in different tones, to fall upon the places of the ſame ſounds. But, ſhould we admit more tones than theſe, as they do, who augment their exceſſes by hemitones, the meſes of two tones muſt, of neceſſity, be applied to the place of one ſound; ſo that, in

in interchanging the tunings of thoſe two tones, the whole ſyſtem in each muſt be removed, not preſerving any one of the preceding tenſions in common, by which to regulate the proper [pitch] of the voice. For the meſe in power of the Hypodorian, for inſtance, being fixed to hypate meſon by poſition, and that of the Hypophrygian to parhypate meſon, the tone taken between theſe two, and called by them the graver Hypophrygian, to diſtinguiſh it from the other acuter one, muſt have its meſe either in hypate, as the Hypodorian, or in parhypate, as the acuter Hypophrygian; which being the caſe, when we interchange the tuning of two ſuch tones, which uſe one common ſound, this ſound is indeed altered a hemitone in pitch, by intention or remiſſion; but having the ſame power in each of the tones, *viz.* that of the meſe, all the reſt of the ſounds are intended or remitted in like manner, for the ſake of preſerving the ratios to the meſe, the ſame with thoſe taken before the mutation, according to the genus common to both tones; ſo that this tone is not to be held different in ſpecies from the former, but the Hypodorian again, or the ſame Hypophrygian, only ſomewhat acuter or graver in its pitch. That theſe ſeven tones, therefore, are ſufficient, and ſuch as the ratios require, be it thus far declared (11).

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(11) Δῆλον δὲ ὅτι, καὶ τούτων μὲν ὑποτεθειμένων ἡμῖν τῶν τόνων, τῆς καθ' ἕκαστον τῇ δυνάμει μέσης, ἰδιός τις γίνεσθαι τὸ διὰ πασῶν φθόγγος, διὰ τὸ ἰσάριθμον αὐτῶν τε καὶ τῶν εἰδῶν. Ἐκλαμβανομένου γὰρ τοῦ διὰ πασῶν κατὰ τὸν μεταξύ πως τοῦ τελείου συνέμπτοντος τόπου τετάρτου, τὸν ἀπὸ τῆς θέσεως τῶν μεσῶν ὑπάτης ἐπὶ τὴν νήτην διεζευγμένων ἕνεκα τοῦ τὴν φωνὴν ἐμφιλοχώρως ἀναστρέφεσθαι καὶ καθαγίνεσθαι περὶ τὰς μέσας μάλιστα μελῳδίας· ὀλιγάκις ἐπὶ τὰς ἄκρας ἐκβαίνουσαν,

With this chapter he ends his doctrine of the modes, and of their reduction; and we ſee he here makes the muſical doctrine of the ſpecies the baſis of his theory, as far as concerns the rejecting the eight redundant modes; though, out of caution, and to obviate objections, he had eſtabliſhed the ſeven upon the harmonic doctrine of the pitch of the

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σαν, διὰ τὸ τῆς παρὰ τὸ μέτειον χαλάσεως ἢ καλατάσεως ἐπίπονον καὶ ζεβιασμένον. Ἡ μὲν τῆ μιξολυδίᾳ μέση κατὰ τήν δύναμιν, ἐφαρμόζεται τῷ τόπῳ τῆς παρανήτης τῶν διεζευγμένων· ἵν' ὁ τόνος τὸ πρῶτον εἶδος, ἐν τῷ πεκειμένῳ, ποιήσῃ τῆ διὰ πασῶν. Ἡ δὲ τῆ λυδίᾳ, τῷ τόπῳ τῆς τείτης τῶν διεζευγμένων, κατὰ τὸ δεύτερον εἶδος. Ἡ δὲ τῆ φρυγίᾳ, τῷ τόπῳ τῆς παραμέσης, κατὰ τὸ τείτον εἶδος. Ἡ δὲ τῆ δωρίᾳ, τῷ τόπῳ τῆς μέσης, ποιῆσα τὸ τέταρλον καὶ μέσον εἶδος τῆ διὰ πασῶν. Ἡ δὲ τῆ ὑπολυδίᾳ, τῷ τόπῳ τῆς λιχανῆ τῶν μέσων, κατὰ τὸ πέμπλον εἶδος. Ἡ δὲ τῆ ὑποφρυγίᾳ, τῷ τόπῳ τῆς παρυπάτης τῶν μέσων, κατὰ τὸ ἕκτον εἶδος. Ἡ δὲ τῆ ὑποδωρίᾳ, τῷ τόπῳ τῆς τῶν μέσων ὑπάτης, κατὰ τὸ ἕβδομον εἶδος. Ὡς τε δύναδαί τινας ἐν τῷ σὺ̈ημαλι τηρεῖδαι φδόγλους ἀκινήτους, ἐν ταῖς τῶν τόνων μεδαρμογαῖς, παραφυλάσσονλας τὸ μέγεδος τῆς φωνῆς· διὰ τὸ μηδέποτε τας ἐν διαφόροις τόνοις ὁμοίας δυνάμεις, τοῖς τῷ αὐτῶν φδόγλων τόποις περιπίπδειν. Πλειόνων δὲ τῶν τόνων παρὰ τέτες ὑποτιδεμένων· ὃ ποιῆσιν οἱ ἐν τοῖς ἡμιλονίοις τὰς ὑπερχὰς αὐτῶν παραύξονλες· ἀναγκαῖον ἔσαι, δύο τόνων μέσας ἑνὸς φδόγλε τόπῳ πανλως ἐφαρμόζειν· ὥς δ' ὅλα κινεῖδαι τὰ συσήματα, κατὰ τὴν εἰς ἀλλήλας τῶν δύο τέτων τόνων μεδαρμογὴν· μηκέτι τηρῆλα κοινήν τινα τὴν ἐξαρχῆς τάσιν, ἢ παραμεληδήσεσαι τὸ ἴδιον τῆς φωνῆς. Τῆς μὲν γὰρ τῆ ὑποδωρίᾳ, φέρε εἰπεῖν, τῇ δυνάμει μέσης, συνεζευγμένης τῇ κατὰ τὴν δέσιν τῶν μέσων ὑπάτῃ· τ δὲ τῆ ὑποφρυγιᾳ τῇ τῶν μέσων παρυπάτῃ· τὸν λαμβανόμενον μεταξὺ τέτων τόνον, καλέμενον δὲ ὑφ' αὐτῶν βαρύτερον ὑποφρύγιον, παρ' ἐκεῖνον, ὀξύτερον, δήσει τὴν αὐτὴν μέσην, ἤτοι κατὰ τὴν ὑπάτην ἔχειν, ὡς καὶ ὁ ὑποδώριος· ἢ κατὰ τ παρυπάτην, ὡς καὶ ὁ ὀξύτερος ὑποφρύγιος. Οὗ συμβαίνονλος, ἐπειδὰν εἰς ἀλλήλας μεδαρμοζώμεδα τὰς τ κοινὸν φδόγλον ἐιληφότας τόνας· κινηδήσεσαι μὲν ὅτος ἐπιταδεὶς ἢ χαλαδεὶς ἡμιλονίῳ· τῷ δὲ τὴν αὐτὴν, ἐν ἑκατέρῳ τῷ τόνων, δύναμιν ἔχειν τετέσι τ μέσης· ἀκολυδήσασιν αἱ τῷ λοιπῶν ἑπτὰ φδόγλων ἐπιτάσεις ἢ χαλάσεις· ἕνεκα τῆ συληρεῖν τὰς πρὸς τ μέσην λόγας, τὰς αὐτὰς τοῖς πρὸ τ μεταβολῆς, κατὰ τὸ κοινὸν ἀμφοτέρων τῷ τόνων γένος, λαμβανομένοις. Ὡς τε μηδ' ἂν ἕτερον ἔτι δόξαι τῷ εἶδει τ τόνον τῷ τ πρότερον· ἀλλ' ὑποδώριον πάλιν, ἢ τ αὐτὸν ὑποφρύγιον, ὀξυφωνότερον τινὸς ἢ βαρυφωνότερον μόνον. Τὸ μὲν οὖν εὔλογόν τε καὶ ἀρχὲς τῷ ἐπὶ τόνων, μέχρι τέτων ὑπολελυπώδω. Ptol. lib. ii. c. 11.

system.

ſyſtem. That the force of his arguments in this chapter, and the reſult of admitting the eight modes, may be more plainly ſeen, I have annexed a ſecond diagram of the ſpecies, as they lay in the ſpurious modes; by which it will appear, to which ſpecies in the genuine ones they ſeverally anſwered; and the objection, raiſed againſt each of them by Ptolemey will become intelligible.

There is, however, an objection, to which this ſecond diagram ſeems liable. It may be aſked, if the Hypoionian mode, inſtanced by Ptolemey, could give either Hypodorian or Hypophrygian harmony, according as either the eighth or the ninth ſtring was employed for its meſe? and if the reſt of the ſpurious modes were liable to a like alternative, what was it determined me in my choice of the two ſpecies, in the conſtruction of the diagram? To which I anſwer, that the antients have decided for me as to five of them; for we find (12), that the Hypoionian was called the graver Hypophrygian; the Hypoæolian, the graver Hypolydian; the Ionian, the graver Phrygian; the Æolian, the graver Lydian; and the Hyperionian, the acuter Mixolydian. And this, by the way, furniſhes us with an unanſwerable argument of the antiquity of the doctrine enforced by Ptolemey, that the number of the modes ought not to exceed that of the ſpecies of the diapaſon; for theſe appellations, which were given to the modes long enough before Ptolemey flouriſhed, can be accounted for on no other principle, but the ſuppoſing the ſpecies of

(12) Vide Euclid. Introduct. Harm. p. 19. v. 29. & ſeq. Ariſt. Quint. p. 23. v. 7. & ſeq.

diapaſon

diapaſon to have been conſidered as the eſſential characters of the modes; and that theſe five modes therefore, though they had a difference in pitch, were thought to be the ſame, as to their effects, with thoſe, after which they were thus named. As to the other three, the Hyperphrygian, Hyperæolian, and Hyperlydian, their meſes being reſpectively at the diſtance of an octave from the Hypodorian, Hypoionian, and Hypophrygian, it was neceſſary to give them the ſame ſpecies with thoſe three modes; for a tranſpoſition of an octave was always underſtood to give a return of the ſame harmony, as has been already ſhewn.

This ſecond diagram, and that of the ſeven genuine modes, may be thought ſufficient to render the ſubject intelligible: however, as the fifteen modes are parted in the two diagrams, and confined alſo to the compaſs of a diapaſon, for ſhewing the ſpecies, I have added a table, ſhewing the tuning of the fifteen ſtrings of the lyre for all the modes. The letters of Guido's ſcale, placed within the ſquares, ſhew, in their horizontal orders, the tunings of all the ſtrings for each mode; the two broader lines include the ſpecies of diapaſon; and the meſes are diſtinguiſhed by capital letters, by which means, the caſes, where the ſame ſtring is employed for the meſes of two modes, appear at the firſt view; and the reaſoning, upon which Ptolemey rejects the eight modes, is illuſtrated, ſo as to need no farther explanation.

Having now gone through with what I propoſed to explain under the three firſt heads, I come,

Fourthly,

R E for every Mode.

a	Dorian.
a#	Hypolydian.
a	Hypoæolian.
a	Hypophrygian.
a♭	Hypoionian.
a	Hypodorian.

A TABLE, ſhewing the Tuning of the STRINGS of the LYRE for every Mode.

STRINGS of the LYRE.		Hyperlydian.	Hyperæolian.	Hyperphrygian.	Hyperionian.	Hypodorian, or Mixolydian.	Lydian.	Æolian.	Phrygian.	Ionian.	Dorian.	Hypolydian.	Hypoæolian.	Hypophrygian.	Hypoionian.	Hypodorian.
15	Nete hyperbolæon.	a	$a^{\flat}$	a	$b^{\flat}$	a	a	$a^{\flat}$	a	$a^{\flat}$	a	$a^{\#}$	a	a	$a^{\flat}$	a
14	Paranete hyperbolæon.	$g^{\#}$	g	g	$a^{\flat}$	g	$g^{\#}$	g	g	$g^{\flat}$	g	$g^{\#}$	g	$g^{\#}$	g	g
13	Trite hyperbolæon.	F$^{\#}$	F	$f^{\#}$	$g^{\flat}$	f	$f^{\#}$	f	$f^{\#}$	f	f	$f^{\#}$	f	$f^{\#}$	f	$f^{\#}$
12	Nete diezeugmenon.	e	$e^{\flat}$	E	f	e	e	$e^{\flat}$	e	$e^{\flat}$	e	e	$e^{\flat}$	e	$e^{\flat}$	e
11	Paranete diezeugmenon.	d	$d^{\flat}$	d	E$^{\flat}$	D	$d^{\#}$	d	d	$d^{\flat}$	d	$d^{\#}$	d	d	$d^{\flat}$	d
10	Trite diezeugmenon.	$c^{\#}$	c	c	$d^{\flat}$	c	C$^{\#}$	C	$c^{\#}$	c	c	$c^{\#}$	c	$c^{\#}$	c	c
9	Parameſe.	b	$b^{\flat}$	b	$c^{\flat}$	$b^{\flat}$	b	$b^{\flat}$	B	B$^{\flat}$	b	b	$b^{\flat}$	b	$b^{\flat}$	b
8	Meſe.	a	$a^{\flat}$	a	$b^{\flat}$	a	a	$a^{\flat}$	a	$a^{\flat}$	A	$a^{\#}$	a	a	$a^{\flat}$	a
7	Lichanos meſon.	$g^{\#}$	g	g	$a^{\flat}$	g	$g^{\#}$	g	g	$g^{\flat}$	g	G$^{\#}$	G	$g^{\#}$	g	g
6	Parhypate meſon.	$f^{\#}$	f	$f^{\#}$	$g^{\flat}$	f	$f^{\#}$	f	$f^{\#}$	f	f	$f^{\#}$	f	F$^{\#}$	F	$f^{\#}$
5	Hypate meſon.	e	$e^{\flat}$	e	f	e	e	$e^{\flat}$	e	$e^{\flat}$	e	e	$e^{\flat}$	e	$e^{\flat}$	E
4	Lichanos hypaton.	d	$d^{\flat}$	d	$e^{\flat}$	d	$d^{\#}$	d	d	$d^{\flat}$	d	$d^{\#}$	d	d	$d^{\flat}$	d
3	Parhypate hypaton.	$c^{\#}$	c	c	$d^{\flat}$	c	$c^{\#}$	c	$c^{\#}$	c	c	$c^{\#}$	c	$c^{\#}$	c	c
2	Hypate hypaton.	b	$b^{\flat}$	b	$c^{\flat}$	$b^{\flat}$	b	$b^{\flat}$	b	$b^{\flat}$	b	b	$b^{\flat}$	b	$b^{\flat}$	b
1	Proſlambanomenos.	a	$a^{\flat}$	a	$b^{\flat}$	a	a	$a^{\flat}$	a	$a^{\flat}$	a	$a^{\#}$	a	a	$a^{\flat}$	a

ed by PTOLEMEY.

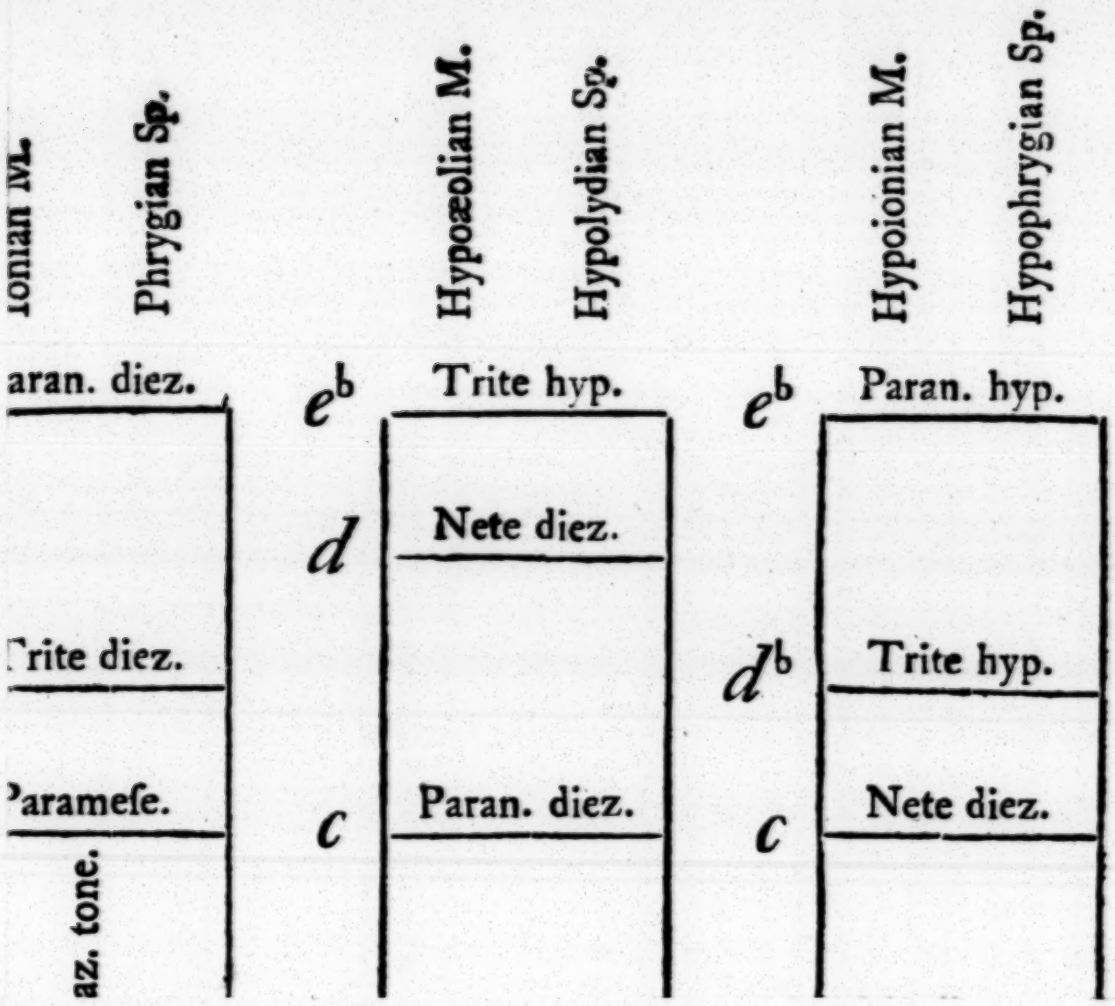
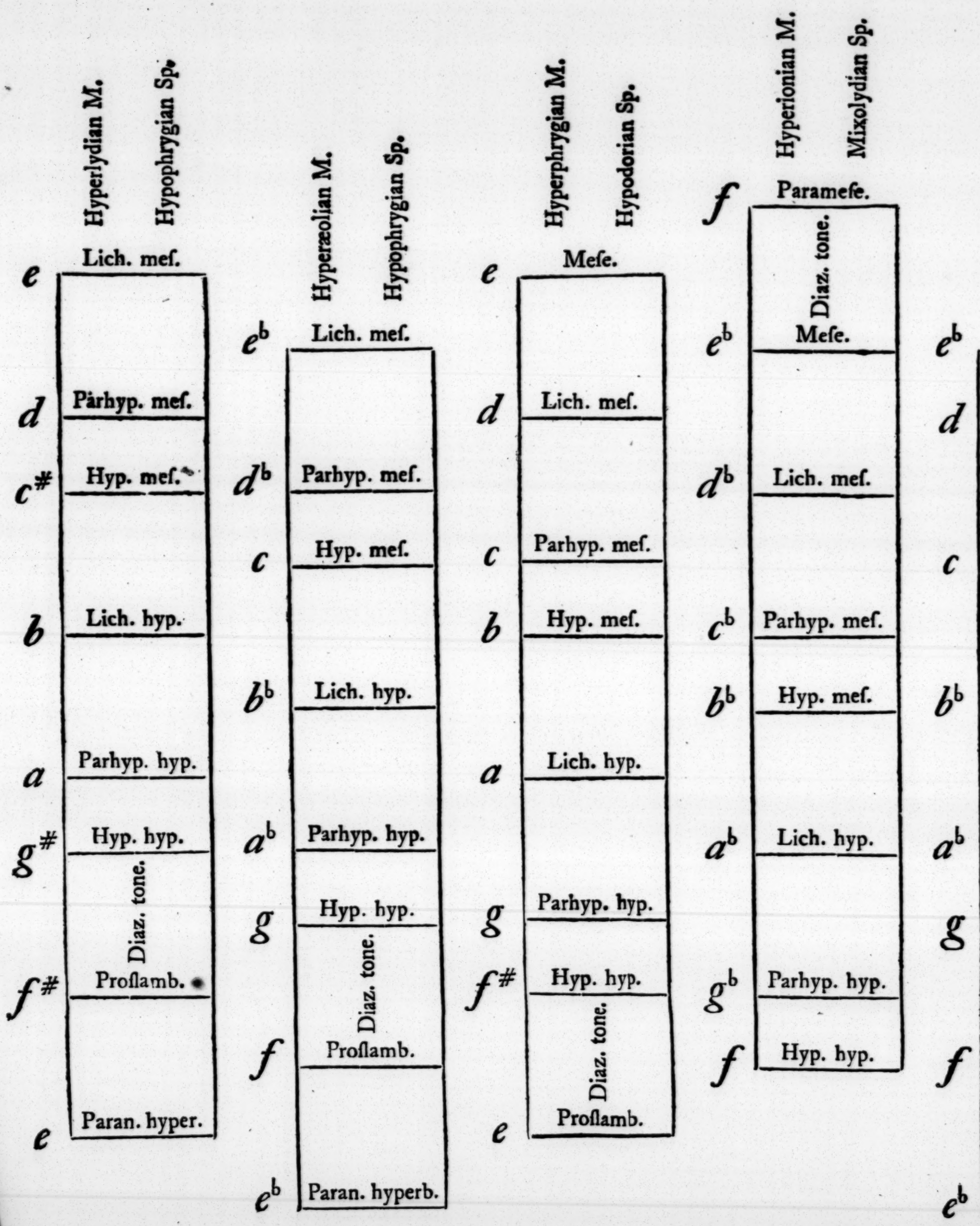


DIAGRAM II. Of the Species of Diapaſon in the



ſon in the Eight Modes rejected by PTOLEMEY.

Æolian M.

e^b	Trite diez.
d	Parameſe.
	Diaz. tone.
c	Meſe.
b^b	Lich. meſ.
a^b	Parhyp. meſ.
g	Hyp. meſ.
f	Lich. hyp.
e^b	Parhyp. hyp.

Lydian Sp.

e^b	Paran. diez.
d^b	Trite diez.
c	Parameſe.
	Diaz. tone.
b^b	Meſe.
a^b	Lich. meſ.
g^b	Parhyp. meſ.
f	Hyp. meſ.
e^b	Lich. meſ.

Ionian M.

e^b	Trite hyp.
d	Nete diez.
c	Paran. diez.
b^b	Trite diez.
a	Parameſe.
	Diaz. tone.
g	Meſe.
f	Lich. meſ.
e^b	Parhyp. meſ.

Phrygian Sp.

e^b	Paran. hyp.
d^b	Trite hyp.
c	Nete diez.
b^b	Paran. diez.
a^b	Trite diez.
g	Parameſe.
	Diaz. tone.
f	Meſe.
e^b	Lich. meſ.

Hypoæolian M.

Hypolydian Sp.

Hypoionian M.

Hypophrygian Sp.

Fourthly, To point out ſomething of the origin of the two doctrines.

The harmonic doctrine was, as I have ſhewn, expreſly taught in harmonic; and therefore, to account for its reception, it will be neceſſary to conſider the ſcope of that ſcience.

The muſical doctrine I have alſo ſhewn to have been explained in harmonic, as far as concerned the elementary principles of the ſpecies of diapaſon; but the application of thoſe principles to the doctrine of the modes was not taught therein, being reſerved for melopœïa, a ſcience, that looked higher than harmonic, and conſidered the uſe of the elements, when in the hands of the poet or muſician. It is to this ſcience, therefore, that we muſt look for the origin of the muſical doctrine.

But, before I conſider the ſcope of either of theſe ſciences, it will be neceſſary to ſhew my reaſons for ſeparating them, ſince they were blended by the Ariſtoxenians, as has been ſhewn above. For this purpoſe, we muſt have recourſe to the diviſion of muſic, the mother ſcience; which, as treated by the antients, comprehended all, that the moderns now underſtand by muſic and poetry. Its firſt general diviſion, according to Ariſtides Quintilianus (13), was into theoretic and practic. The theoretic was ſubdivided into natural and artificial; the latter of which was again ſubdivided into harmonic, rhythmic, and metric;

(13) De Muſica, p. 7. See alſo Meibomius on the paſſage, who has embraced the diviſion of Ariſtides, in the following table.

metric; which three ſciences more particularly contained the poets elements, as teaching the grounds of tune, time, and verſe. But nothing leſs than a competent knowlege of every branch of the mother ſcience could carry with it the reſpected title of ὁ μυσικος, the muſician. To harmonic, rhythmic, and metric, in the theoretic, reſpectively anſwered melopœïa, rhythmopœïa, and poetic, in the practic. In this author's diviſion therefore, we ſee, that harmonic and melopœïa are diſtinguiſhed, even by the firſt general diviſion. Of the propriety of this even the Ariſtoxenians ſeem to have been ſenſible; ſince, in order to juſtify their blending them, they have defined harmonic as a ſcience, both theoretic and practic (14). And Ariſtoxenus himſelf, in the fragments we have under the falſe title of his three books of harmonic elements, ſeems to affign ſuch bounds to harmonic, as might well be underſtood to exclude

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MUSICÆ
- alia eſt pars **Theoretica**, cujus rurſus partes duæ
  - **Phyſica**, quæ dividitur in
    - Arithmeticam,
    - Phyſicam generi cognominem.
  - **Artificialis**, quæ dividitur in
    - Harmonicam,
    - Rhythmicam,
    - Metricam.
- alia **Practica**, cujus item partes duæ
  - **Uſualis**, cujus partes
    - Melopœïa,
    - Rhythmopœïa,
    - Poeſis.
  - **Enarrativa**, cujus partes
    - Organica,
    - Odica,
    - Hypocritica.

(14) Ἁρμονική ἐστιν ἐπιστήμη θεωρητικὴ καὶ πρακτικὴ τῶν πρὸς μέλος φύσεως. Euclid. Introd. Harmon. initio.

melopœïa

melopœïa (15). We have Plutarch alſo on our ſide, who, in a paſſage of his dialogue on muſic, which I ſhall have occaſion to cite, amongſt other proofs, enumerates only the firſt ſix parts of harmoniac, leaving melopœïa out of his diviſion. For this Meibomius blames him (16), but too haſtily; for had the learned critic conſidered the whole paſſage, he muſt have ſeen, that the omiſſion was not through ignorance, but deſign, the author being there expreſly arguing for the confining of harmonic within its proper bounds. However, whether the Ariſtoxenians were right or wrong, in including melopœïa in harmonic, my argument ſeems not materially affected by it; for they gave it only as a laſt diviſion, to be taught after all the reſt had been inculcated; and they agreed in defining it to be the uſe of the harmonic elements, according to the propriety of each ſubject (17): which is diſtinction enough for the uſe I propoſe to make of it.

Having thus far juſtified my dividing theſe ſciences, I ſhall now conſider the ſcope of each; which

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(15) Τῆς περὶ μέλους ἐπισήμης πολυμερῶς ὔσης, καὶ διηρημένης εἰς πλείους ἰδέας, μίαν τινὰ αὐτῶν ὑπολαβεῖν δεῖ, τὴν ἁρμονικὴν καλουμένην, εἶναι πραγματείαν, τῇ τε τάξει πρώτην οὖσαν, ἐχούσάν τε δύναμιν συ-χειώδη. Τυγχάνει γὰρ οὖσα πρώτη τῶν θεωρητικῶν· ταῦτα δέ ἐστιν ὅσα συντείνει πρὸς τὴ τῶν συστημάτων τε καὶ τόνων θεωρίαν. Προσήκει γὰρ μηδὲν προῤῥωτέρω τούτων ἀξιοῦν παρ' αὐτῷ τὸν τὴν εἰρημένην ἔχοντ' ὄφελιμον· τέλος γὰρ τοῦτό ἐστι τῆς πραγματείας ταύτης. Ariſtox. p. 1. init.

(16) Plutarchus dialogo de Muſica ſex tantum priores turbato etiam ordine recenſet, cum tamen crebram μελοποιΐας mentionem injiciat, adeoque, non una ratione excuſari illud poteſt. Not. in Gaudientium, p. 30.

(17) Μελοποιΐα δέ ἐστι χρῆσις τῶν ὑποκειμένων τῇ ἁρμονικῇ πραγματείᾳ, πρὸς τὸ οἰκεῖον ἑκάστης ὑποθέσεως. Euclid. Introd. Harmon. p. 2. v. 5. & p. 23. v. 18.

will lead us to the origin of the doctrines they contained.

The object of melopœïa was propriety in the uſe of the harmonic elements, as appears from the definition juſt cited. In what this propriety conſiſted, it is difficult to know, as no collection of the rules of this ſcience is come down to us: it is not to be learnt from the harmonic writers, who treat very ſlightly of this branch; and thoſe of them, who are the fulleſt upon it, as Ariſtides Quintilianus and Manuel Briennius, wrote in later times, when the faſhion of muſic was changed, and a new kind of melopœïa prevailed, with the rules of which the earlier muſicians could hardly have been acquainted; as, on the other hand, many of the earlier rules muſt have been forgot, or, in a great meaſure, diſuſed. There is more light to be procured from the dialogue of Plutarch, and ſome others, who ſpeak occaſionally of the practice of muſic. I do not propoſe to collect any thing here upon this ſubject, as, indeed, there does not ſeem to be ſufficient materials left us for a thorough explanation of it, which would require no leſs than a complete hiſtory of the ſcience of its muſic, and its improvements: it is ſufficient for my purpoſe to ſay, that, in the earlier times, when the beſt muſic is ſuppoſed to have prevailed, the genera, modes, rhythmi, metres, *&c.* were not employed indiſcriminately, nor even left to the diſcretion of the muſician, but were under particular reſtrictions, which confined them to the arguments, to which they were held reſpectively the beſt adapted; and that it was from the mixture of theſe ingredients, that aroſe the ethic character of the compoſition. Hence the ſeveral

nomi

nomi took their riſe; of which there is, in the dialogue of Plutarch, an ample account. As what I am now advancing cannot but be well known, I need look no farther for the proof of it, than to a paſſage of Plato's third book of laws, where he complains of a licence beginning even in his time to the prejudice of the ſcience. Speaking of times paſt,

Our muſic (ſays he) was then divided according to certain ſpecies, and figures thereof. Prayers to the gods were one ſpecies of ſong, to which they gave the name of hymns: oppoſed to this was another ſpecies, which, in particular, might be called threni; another, pæones; and another, the birth of Dionyſius, which I hold to be the dithyrambus: there were alſo citharœdic nomi, ſo called, as being ſtill another ſong. Theſe, and ſome others, being preſcribed, it was not allowable to uſe one ſpecies of melos for another.——But afterwards, in proceſs of time, the poets firſt introduced an unlearned licence, being poetic by nature, but unſkilled in the rules of the ſcience, trampling upon its laws, over attentive to pleaſe, mixing the threni with the hymns, and the pæones with the dithyrambi, imitating the muſic of the flute upon the cithara, and confounding all things with all, *&c.* (18).

This

(18) Διηρημένη γὰρ τότε ἦν ἡμῖν ἡ μουσικὴ κατ' εἴδη τε ἑαυτῆ ἅπαντα καὶ σχήματα· καὶ τί ἦν εἶδος ᾠδῆς εὐχαὶ πρὸς θεούς· ὄνομα δὲ ὕμνοι ἐπεκάλουν· καὶ τούτῳ, δὲ τὸ ἐναντίον ἦν ᾠδῆς ἕτερον εἶδος· θρῆνος δὲ τις ἂν αὐτὸς μάλιστα ἐκάλεσε· καὶ παιᾶνες, ἕτερον καὶ ἄλλο Διονύσου γένεσις, οἶμαι, διθύραμβος λεγόμενος· νόμους τε αὐτὸ τοῦτο τοὔνομα ἐκάλουν ᾠδὴν ὥς τινα ἑτέραν ἐπέλεγον δὲ κιθαρῳδικούς. Τούτων δὲ διαπεπλεγμένων καὶ ἄλλων τινῶν οὐκ ἔξην ἄλλῳ εἰς ἄλλο καταχρῆσθαι μέλους εἴδει. —Μετὰ δὲ ταῦτα ποιητὴν τῆς χορείας, ἀρχοντες ἐπὶ τῶν ἀμούσων νομίσαι

This paſſage, with what has been ſaid, being ſufficient to give a general idea of the ſcope of melopœïa, I ſhall paſs to that of harmonic.

We have already ſeen, that the theoretic diviſion of muſic was by the author, whoſe diſtribution I have followed, ſubdivided into natural and artificial, and that harmonic came under the latter. The antients, indeed, ſeem not all to have treated the ſcience under ſuch narrow limits, the phyſical properties of ſound, the ratios of intervals, *&c.* appearing to have been conſidered therein, by the Pythagorean and other ſchools, as well as the ſtructure of the ſyſtems in uſe. But not to ſtop at this objection, which is not very material, thus far muſt be allowed, that harmonic, as to the greateſt part of its ſcope, was an artificial ſcience, its moſt conſiderable object being to explain and teach the denominations, poſitions, powers, *&c.* of the ſounds, of the ſyſtem, and all other inventions in uſe, for facilitating the ſtudy of muſic, or bringing it to greater perfection. Now, in this ſhort view of the ſcience, which is ſufficient for our purpoſe, we may already begin to account for the difference between its doctrines and thoſe of melopœïa, artificial ſciences being but too prone to admit doctrines for convenience, that are repugnant to the truth. This was indeed the caſe with harmonic, as the two doctrines of the tones ſufficiently teſtify; the origin of which I ſhall now conſider.

νομίας ποιηταὶ ἐγίγνοντο, φύσει μὲ ποιητικοὶ, ἀγνώμονες δὲ περὶ τὸ δίκαιον τ μέσης καὶ τὸ νόμιμον, βακχεύοντες, καὶ μᾶλλον τῶ δέονθ' καθεχόμενοι ὑφ' ἡδονῆς, κεραννῦντες δὲ θρήνοισε ὕμνοις καὶ παιῶνας διθυράμβοις καὶ αὐλωδίας δὴ τ κιθαρωδίαις μιμούμενοι, καὶ πάντα εἰς πάντα ξυνάγοντες. Plat. de Legibus, lib. iii.

The

The muſical doctrine I look upon as the moſt antient, and that which exhibited the modes in their proper character; for which reaſon, it could not avoid making a part, and probably the moſt eſſential one, of the doctrines of melopœïa. The preciſe time when it firſt came into uſe, I pretend not, in the midſt of uncertainties, to be able to determine; but we may, perhaps, be near the truth, if we refer its origin to the age of Pythagoras, if the eighth ſtring of the lyre was really, as we are told, the addition of that philoſopher. The ſtory of his diſcovering, at a forge, the ratios of hypate, meſe, parameſe, and nete, is well known. Theſe ratios, which gave riſe to the harmonic proportion, and the numbers of which were ſo vainly applied afterwards, by philoſophers, to all the parts of the univerſe, were drawn from the Dorian ſpecies of diapaſon, which we cannot ſuppoſe to have been well underſtood, till its form exiſted upon the inſtrument. What doctrine of the modes prevailed, before the addition of the eighth ſtring, we can only gueſs at; for it was a queſtion, even with Ariſtotle (19), how the heptachord lyre had been adjuſted; and Nichomachus ſpeaks of that inſtrument ſo

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(19) Διὰ τὶ οἱ ἀρχαῖοι ἑπταχόρδους ἁρμονίας ποιοῦντες τὴν ὑπάτην, ἀλλ' ὀ τὴν νήτην κατέλιπον; πότερον τοῦτο ψεῦδος, ἀμφοτέρας γὰρ κατέλιπον, τὴν δὲ τρίτην ἐξήρων; ἢ ὅ, ἀλλ' ὅτι ἡ βαρυτέρα ἰσχύει τῆ τῆ ὀξυτέρας φθόγγον, ὥστε μᾶλλον ἡ ὑπάτη ἀποδίδοσι τὸ ἀντίφωνον ἢ ἡ νήτη· ἐπεὶ τὸ ὀξὺ δυνάμεως μᾶλλον, τὸ δὲ βαρὺ ῥᾷον φθέγξασθαι. Ariſtot. Probl. ſect. 19. qu. 7.

Διὰ τὶ οἱ ἀρχαῖοι ἑπτάχορδους ποιοῦντες τὰς ἁρμονίας τὴν ὑπάτην ἀλλ' ὀ τὴν νήτην κατέλιπον; ἢ ὅ τὴν ὑπάτην, ἀλλὰ τὴν νῦν καλουμένην παραμέσην ἀφῆρων καὶ τὸ τονιαῖον διάστημα, ἐχρῶντο δὲ τῆ ἐσχάτη μέσῃ τῶν ἐπὶ τὸ ὀξὺ φωνῶν, διὸ καὶ μέσην αὐτὴν προσηγόρευσαν; ἢ ὅτι ἦν τῶν μὲν ἄνω τετραχόρδων τελευτὴ, τῶν δὲ κάτω ἀρχὴ, καὶ μέσον ἔχει λόγον τόνῳ τῶν ἄκρων. Probl. ſect. 19. qu. 48.

confuſedly,

confuſedly, as to have given room for a ſuppoſition (20), that it had received two forms, the ſeven ſtrings anſwering, in the oldeſt form, to our notes *e*, *f*, *g*, *a*, *b*♭, *c*, *d*, and in the new one, to *e*, *f*, *g*, *a*, *b*, *d*, *e*. However this may have been, neither the muſical nor the harmonic doctrines could be then in uſe, exactly as they were taught in after times; and the probability ſeems to be, that the modes were in thoſe days characterized by the ſpecies of the leſſer conſonances, diateſſaron and diapente; but the theory of the Dorian ſpecies of diapaſon, we may be ſure, took place about the time, when the lyre was brought to that compaſs; and the other ſpecies, though they might have exiſted before, in the melody of particular inſtruments; as for inſtance, the barbarous Phrygian, upon its flute, could hardly have been taken into the Greek theories of the ſcience, till the extenſion of their own favourite inſtrument had brought the diapaſon under conſideration: ſo that the origin of the muſical doctrine of the modes, is, with great probability, to be referred to this improvement of the lyre.

I come now to the harmonic doctrine, for the origin of which we muſt look to the invention of the ſyſtem. The greater perfect ſyſtem, upon whoſe pitch the modes depended, by the harmonic doctrine, conſiſted, as I have already ſhewn, of fifteen ſounds, anſwerable to thoſe of the lyre; and it is reaſonable

(20) Ut ex his difficultatibus nos expediamus, duas, non opiniones, ſed ætates ſtatuere debemus, quibus aliter obtinuerint intervalla in ſeptem chordis. Meibomius in Nichomachum, p. 52. See alſo Nichomachus, p. 9. v. 14. & ſeq. & p. 17. v. 24. & ſeq.

to think, that the ſtructure of it was not ſettled, till the inſtrument had been again extended to the compaſs of a diſdiapaſon, by the additions of the tetrachords hyperbolæon and hypaton, and of the ſound proſlambanomenos: for this change the age of Alexander the Great may, perhaps, be a probable æra; for, in the muſical problems of Ariſtotle, I obſerve no mention of the new tetrachords, though many of the queſtions concern the ſtrings of the lyre; and yet there is no doubt of their being in uſe in the time of Ariſtoxenus, his diſciple. Should I be near the truth in this, the muſical doctrine will then appear to have been earlier than the harmonic, by the whole period from Pythagoras to Alexander. But, without being ſollicitous about the preciſe time when the harmonic doctrine was introduced, I ſhall, with more certainty, endeavour to point out what muſt have given occaſion to it.

The ſtudy of the muſic of the antients, though they ſeem not to have much uſed compoſition in parts, muſt yet have been very perplexing, from the variety only of the tones and genera; and ſome help might well be thought neceſſary, even in the time of the octachord lyre. But when ſeven new ſtrings were given to the inſtrument, and theſe placed not all at one end, but three at the acute, and four at the grave, the eight old ſtrings, upon which the characteriſtic ſpecies of melody for each mode had been always exhibited, became confounded by theſe additions; and poſſeſſing now the center of the lyre, it was difficult to diſtinguiſh them, and to preſerve for each mode its proper character as before. This difficulty muſt alſo have been increaſed, by the change

introduced

introduced in the practice of the ſcience; for both the players and compoſers, having now a greater latitude, would not fail to take advantage of the enlargement; and though the ſkilful among them might, in their excurſions upon the new ſtrings, preſerve a due attention to the proper characters of the ſeveral modes, yet the melody peculiar to each could not be ſo eaſily comprehended, when carried beyond its uſual limits. An artificial help, therefore, to the learner, was now become indiſpenſably neceſſary; and with this view, I make no doubt, but that the ſyſtem was invented. It was, indeed, admirably well contrived for the purpoſe; for its ſucceſſion being the ſame with that of the lyre, in its Dorian tuning, with which the Greeks muſt have been the beſt acquainted, as being the proper mode of their favourite inſtrument, it was the moſt eaſy to be learnt and retained, and the knowlege of this ſucceſſion was all now required; for, by imagining only the ſyſtem removed to ſome other pitch, and tuning all the ſtrings in the ſame relations to a new meſe, the melody of the old eight ſtrings was thereby changed, and a new ſpecies of diapaſon gained thereon, without the trouble of ſtudying the the muſical doctrine, which was now left to the muſician, or meſopœïus, who was anſwerable for the choice of the ſucceſſions he ſelected for his various ſubjects, whilſt the harmonician followed him through the intricacies of his compoſitions, by the eaſier method of the tranſpoſition of the ſyſtem. And this I apprehend to have been the origin, both of the ſyſtem itſelf, and of the harmonic doctrine of the tones. But now, as it often happens, what was deſigned for the improvement of the ſcience, became,

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in time, the ruin of it; for, after the reception of the diſdiapaſon ſyſtem, the elementary treatiſes, as we ſee by what is left to us of them, became filled with this new doctrine of the parts and relations of the ſyſtem, of its tranſpoſitions, of the poſitions of the meſe, of ſounds in power, and ſound by poſition, and many other doctrines flowing from the ſame ſource, till, in time, the ſyſtem itſelf came to be conſidered as the true type of a mode; and a number of new modes were introduced, that were grounded on no other principle than this, which I ſcruple not to call a falſe doctrine of them, though the antients admitted it for convenience, as I have ſhewn, and thereby almoſt loſt their impreſſions of the new one.

Having thus pointed out the origin of theſe two doctrines, as far as was poſſible, from a general view of them, and without entering into a critical examination of the many paſſages of the antients, that might help us to greater certainty, I ſhall now proceed,

Fifthly, To ſhew how far the preceding explanations may be ſupported by arguments, or warranted by the teſtimony of antient writers.

Here I muſt repeat, that the harmonic doctrine of the tones, as I have explained it, is found, expreſly delivered under its proper head, in almoſt every writer on the ſubject; and that the doctrine of the ſpecies of diapaſon is found alſo in the harmonic treatiſes, though not under the head of tones, nor expreſly applied to them. Now, this being the caſe, I ſhall have no occaſion to waſte time in bringing

particular proofs, to ſupport the explanations I have given of theſe two doctrines ſeparately. What immediately lies upon me is, to prove, that the doctrine of the ſpecies of diapaſon had a connection with the modes; and to juſtify the manner, in which I have combined the two doctrines in the canon, and in the diagrams framed from it. With this view, therefore, I ſhall direct my arguments to the proving of the five following points.

1. That the doctrine of the ſpecies of diapaſon was a doctrine of the modes.

2. That this doctrine was not diſtinct from, nor independent of, the harmonic, but ſo connected with it, that a mutation, according to the one, produced the alteration required by the other.

3. That the ſpecies of diapaſon, as taken practically on the lyre, lay all at the ſame pitch.

4. That this pitch was the diapaſon, between the ſtrings hypate meſon and nete diezeugmenon, as they ſtand in the diagram.

5. I ſhall endeavour to ſhew, that it was the muſical doctrine, and not the harmonic, that exhibited the true character of the modes, as conſidered in reſpect to their effects.

To begin with the firſt point. That the doctrine of the ſpecies of diapaſon was a doctrine of the modes, might, perhaps, be inferred from ſome of the definitions given of a mode; but as theſe definitions are moſtly intermixed with, if not drawn ſolely from, the harmonic doctrine, and hence have given room for diſputes, I ſhall reſt this point upon three arguments, in which the definitions are not concerned.

Firſt,

Firſt, Becauſe the ſpecies are denominated after ſeven of the modes, which could hardly have ariſen from any other cauſe, than their connection with them.

2dly, Becauſe the whole reaſoning of Ptolemey, in the ſixth, ſeventh, eighth, ninth, tenth, and eleventh chapters, of his ſecond book, tends to reduce the modes to the number of the ſpecies of the diapaſon; which, as far as his opinion goes, is an expreſs confirmation, that the muſical doctrine was a doctrine of the modes.

3dly, Becauſe Ptolemey is not ſingle in this opinion, as ſome may have thought, but is ſupported in it by the Ariſtoxenian writers, who, though they admitted thirteen modes, upon the falſe principle of the harmonic doctrine, did, in fact, reduce them to eight, on the very principle of Ptolemey, calling the Hyperionian mode, which gave the ſame ſpecies with the Mixolydian, the acuter Mixolydian; and calling alſo the Æolian, the graver Lydian; the Ionian, the graver Phrygian; the Hypoæolian, the graver Hypolydian; and the Hypoionian, the graver Hypophrygian; the former of all which modes gave, reſpectively, the ſame ſpecies with the latter. So that, to make their doctrine anſwer to that of Ptolemey, there wanted but the reduction of one mode more, which was their Hyperphrygian. And it is not to be conceived, but that they ſaw this mode alſo to be a repetition of the Hypodorian; but as this repetition was at the diſtance of an octave, and the other five were repeated at the difference only of a ſemitone in pitch, they could not have called this the acuter Hypodorian, without uſing the ſame expreſſion in too great a lati-

tude of ſenſe. And this might be the reaſon why they omitted taking notice of that circumſtance attending this mode; though we might perhaps, with ſafety to the argument, go a ſtep farther, and ſuppoſe, that the Ariſtoxenians might think a difference of an octave in pitch worthy of a diſtinct denomination, though they would not allow it to that of a ſemitone. And this is the more probable, as we find they did not abſolutely reject theſe modes, as Ptolemey did, but admitted them upon the principles of the harmonic doctrine, though, by their denominating five of them from the genuine modes, it is plain they acknowleged them to be ſuperfluous, in reſpect to the muſical. I cannot ſay more to this point, without defrauding the ſubſequent ones; and I ſhall therefore leave it, with this remark, that whatever is proved of the ſubſequent points, and eſpecially of the fifth, muſt hold true of this, *à fortiori*; ſo that it may really be ſaid to have the ſupport of the whole teſtimony.

2. To the ſecond point, the firſt diagram ſpeaks very ſtrongly; for had the two doctrines no relation to each other, no ſuch coincidence, as is there found, could have been expected, in the reſult of their combination; and this proof, from the coincidence of the doctrines in the diagram, becomes much ſtronger, if we attend to theſe two circumſtances. Firſt, That in the canon, upon which the diagram has been conſtructed, there is no ſtrain of either doctrine, but a plain and natural combination of them, as they are ſtated ſeparately by the antient writers. And ſecondly, That the ſpecies of diapaſon, as brought out by this combination, all fall upon the ſtrings of the old

octachord

octachord lyre, where it was natural to expect them; for when the lyre had but eight ſtrings, the ſpecies could be taken no where elſe; and it cannot be thought, but that, after the extenſion of the inſtrument, they were taken at the ſame pitch as before, unleſs we ſhould ſuppoſe, that the new ſtrings brought with them an immediate change, both of the theory and practice of the ſcience, which is moſt improbable. We ſee our modern harpſichords have more keys given to them than thoſe of the laſt century; and yet neither the poſitions of the cliffs, nor any other circumſtances attending our theories of the ſcience, have been altered by them. But that this point may not reſt wholly on the circumſtances of the diagram, I ſhall produce four paſſages, that plainly ſhew the relation between the two doctrines. The firſt from Ariſtoxenus.

"Now, as ſome of thoſe [ſyſtems] which we employ in muſic, are ſimple, and others mutable, we muſt treat of mutation; and firſt of this, what mutation is, and how it is accompliſhed; now, I ſay, there happens, as it were, a pathos in the melody; afterwards, how many mutations there are in all, and according to what intervals (21)."

In this paſſage, the author is ſpeaking of the ſixth diviſion of harmonic, which was mutation; and more particularly of mutation with reſpect to tone, which, in this diviſion, was always treated upon the

(21) Ἐπεὶ δὲ τῶν μελῳδουμένων ἐστὶ τὰ μὲν ἁπλᾶ, τὰ δὲ ἀμετάβολα, [corrige, ἐμμετάβολα] περὶ μεταβολῆς ἂν εἴη λεκτέον· πρῶτον μὲν αὐτὸ, τί ποτ' ἐστὶν ἡ μεταβολὴ, καὶ πῶς γινόμενον. Λέγω δ' οὖ πάθους τινὸς συμβαίνοντος ἐν τῇ τῶ μελῳδίας τάξει. Ἔπειτα πόσαι εἰσὶν αἱ πᾶσαι μεταβολαὶ, καὶ κατὰ πόσα διασήματα. Ariſtox. p. 38. v. 7.

principle

principle of the harmonic doctrine, and is ſo here; but he tells us, there was a pathos in the melody, which can only relate to the muſical doctrine, and therefore ſhews their connection. In the next paſſage, from Ariſtides Quintilianus, it is more plainly hinted at.

" For if a certain type of the voice follows each ſyſtem, it is manifeſt, that the ſpecies of the melos will be altered with the harmonics (22)."

But the two following paſſages from Ptolemey will put the matter out of diſpute.

" For we are not to imagine this conſtitution of the mutation, according to the tone, eſtabliſhed for ſake of graver or acuter voices, (ſince the intenſion, or again the remiſſion of whole inſtruments, ſuffices for ſuch a difference, no alteration being produced in reſpect of the melos, the whole being executed by performers of graver or acuter voices); but with this view, that the ſame melos, begun by the ſame voice, now from acuter places, and now from graver, may produce a certain change of ethos; becauſe that in the permutations of the tones, the extremities of the voice do not anſwer to both the extremities of the melos; but in one, the extremity of the voice always falls ſhort of that of the melos; and in the other, the extremity of the melos of that of the voice: ſo as that the ſame melos, which at firſt (meaning in the Dorian) anſwered to the compaſs of the voice, now falling ſhort

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(22) Εἰ γὰρ ἑκάστῳ συστήματι καὶ ποιός τις ἐπακολουθεῖ τῆς φωνῆς τύπος δῆλον ὡς ἅμα τῇ ἁρμονίαις καὶ τὸ τοῦ μέλους εἶδος ἀλλοιωθήσεται. Ariſtid. Quintil. p. 24. v. ult.

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of it, and now again exceeding it, may give to the ear the impreſſion of another mode (23)."

" For of the mutations in reſpect to tone, ſo called, there are two principal differences, the one by which we run through the whole melos in an acuter tenſion, or again in a graver, preſerving always the ſucceſſion of the ſpecies. The other, by which there is an alteration, not of the whole melos in its tenſion, but only of a part thereof, from the ſucceſſion it ſet out with. Wherefore, this laſt might be called rather a mutation of the melos, than of the tone; for, according to the former, the melos is not altered, but the tone (tenſion) of the whole; whereas, according to the latter, the melos varies from its proper order, and the tenſion varies not as tenſion, but as on account of the melos. Hence the former does not ſtrike the ſenſe with an idea of that alteration, according to power, by which the ethos is changed, but only of an alteration in reſpect to the being acuter or graver; whereas the latter cauſes the ſenſe to drop from the uſual and expected melos, the ſucceſſion being preſerved ſome time, and then making a tran-

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(23) Οὐδὲ γὰρ ἔνεκεν τῶν βαρυτέρων ἢ ὀξυτέρων φωνῶν ἔυρυθμόν ἂν τὴν σύςασιν τὴν κατὰ τὸν τόνον μεταβολῆς γεγεννημένην· ὁπότε πρὸς τὴν τοιαύτην διαφορὰν ἡ τῶν ὀργάνων ὅλων ἐπίτασις, ἢ πάλιν ἄνεσις ἀπαρκεῖ, μηδεμιᾶς γε ἀλλαγῆς περὶ τὸ μέλος ἀποτελυμένης, ὅταν ὅλον ὁμοίως ὑπὸ τῶν βαρυφωνοτέρων ἢ τῶν ὀξυφωνοτέρων ἀγωνισῶν διαφαίνηται· ἀλλ' ἕνεκα τῆς κατὰ τὴν μίαν φωνὴν τὸ αὐτὸ μέλος ποτὲ μὲν ἀπὸ τῶν ὀξυτέρων τόπων ἀρχόμενον, ποτὲ δὲ ἀπὸ τῶν βαρυτέρων, τερπήν τινα τῆς ἤθους ἀποτελεῖν τῷ μηκέτι πρὸς ἑκάτερα τὰ πέρατα τῆς μέλους συναπαρτίζεσθαι τὰ τῆς φωνῆς ἐν τοῖς τόνων ἐναλλαγαῖς· ἀλλ' ἀεὶ περικαλαλήγειν, ὅτι μὲν θάτερα, τὸ τῆς φωνῆς πέρας τῆς τοῦ μέλους· ὅτι δὲ τὰ ἐναντία, τὸ τοῦ μέλους πέρας τῆς τῆς φωνῆς· ὥστε τὸ ἐξαρχῆς ἐφαρμόσαν τῇ διασάσει τῆς φωνῆς μέλος, πῇ μὲν ἀπολεῖπον ἐν τοῖς μεταβολαῖς, πῇ δὲ ἐπιλαμβάνον, ἑτέρου ἤθους φαντασίαν παρέχει τοῖς ἀκροαῖς. Ptol. Harm. lib. ii. c. 7.

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ſition to another ſpecies, whether according to genus or tenſion (24)."

3. The third point will receive much ſupport from theſe two conſiderations; firſt, That diapaſons, variouſly divided and variouſly bounded, alſo ſeem to form a more complicated theory, than can well be ſuppoſed to have prevailed in early times, when muſic had not yet loſt its natural ſimplicity; and ſecondly, That the compaſs of the inſtrument, in its octachord ſtate, would not admit of any variation, in reſpect to the bounding ſounds. But there is teſtimony to this point alſo: I ſhall cite one pretty remarkable paſſage, from Ariſtides Quintilianus, which will be ſufficient, as the proof of the next point neceſſarily carries this along with it. Ariſtides, who, in his explanations of the harmonic doctrines, follows the method of the Ariſtoxenians, coming under the head of ſyſtems, to treat of the ſpecies of diapaſon, after explaining their ſituation in the immutable ſyſtem, and aſſigning their denominations, immediately ſubjoins:

" Hence it is plain, that if we make our firſt ſign the ſame, though denominated in different caſes from

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(24) Εἰσὶ δ' αἱ, παρὰ τὸ ἕτω λεγόμενον τόνον, μεταβολῶν δύο πρῶται διαφοραί· μία μὲν καθ' ἣν ὅλον τὸ μέλος ὀξυτέρᾳ τάσει διέξιμεν, ἢ πάλιν βαρυτέρᾳ, τηρήσεις τὸ διὰ παντὸν τῆς εἴδους ἀκόλουθον· δευτέρα δὲ, καθ' ἣν ἁ ὅλον τὸ μέλος ἐξαλλάσσεται τῇ τάσει, μέρος δέ τι παρὰ τὴν ἐξαρχῆς ἀκολουθίαν· διὸ αἱ καλοῖτ' ἂν ἄυλη, τῆς μέλους μᾶλλον, ἢ τῆς τόνου μεταβολή. Κατ' ἐκείνην μὲν γὰρ ἁ ἀλλάσσεται τὸ μέλος, ἀλλ' ὁ διόλου τόνος· κατὰ ταύτην δ', τὸ μὲν μέλος ἐκπίπτει τὰς οἰκείας τάξεως· ἡ δὲ τάσις, ἁ ὡς τάσις, ἀλλ' ὡς ἕνεκα τῆς μέλους· ὅθεν ἐκείνη μὲν ἁ ἐμποιεῖ τοῖς αἰσθησεσι φαντασίαν ἑτερότητος τὰς κατὰ τὴν δύναμιν, ὑφ' ἧς κινεῖται τὸ ἦθος, ἀλλὰ μόνης τῆς κατὰ τὸ ὀξύτερον ἢ βαρύτερον· ἄυλη δ' ὥσπερ ἐκπίπτειν αὐτὴν ποιεῖ τῆς συνήθους καὶ προσδοκωμένης μέλους, ὅταν ἐπὶ πλέον μὲν συνείρηται τὸ ἀκόλουθον, μεταβαίνῃ δὲ πρὸς ἕτερον εἶδος, ἤτοι κατὰ τὸ γένος, ἢ κατὰ τὰ τάσιν. Ptol. Harm. lib. ii. cap. 6.

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a different power of ſound, the quality of the harmony will become manifeſt, from the ſucceſſion of the ſounds in order (25).

In this paſſage, it is to be obſerved, that the term σημειον, a ſign, ſignifies a muſical character, ſuch as the antients uſed to write over the words of a ſong, to expreſs the air, or tune; and that each of theſe characters repreſented not a ſound of the ſyſtem, ſubject to tranſpoſition, but, like the notes of our gamut, a ſound of a certain pitch, or at leaſt liable to very ſmall variations only, from the generic differences. The term, therefore, is properly uſed here by Ariſtides, in oppoſition to φθογγος, a ſound of the ſyſtem, whoſe pitch depended on the mode in which it was employed. As to the expreſſion, quality of the harmony, it evidently reſpects the ſpecies of diapaſon; ſo that the author plainly means to tell us, that if we begin a diapaſon always with the ſame note, the ſucceſſion of its ſounds, that is, the manner in which it is divided, will determine the ſpecies: and ſo far he ſpeaks to our purpoſe. But this point will receive farther proof, from what we ſhall ſay upon the next.

4. The fourth point being the natural reſult of the combination of the two doctrines, carries a ſort of conviction with it; and the ſtronger, as not only the ſeven modes, but the whole fifteen, aſſiſt the proof, as will appear, on examining the table I have given of them: for there the ſounds lying between the

(25) Ἐκ δὴ τούτου φανερὸν, ὡς ἡ ταυτὸν ὑποδεμένοις σημεῖον πρῶτον, ἄλλῃ ἄλλῃ δυνάμει φθόγγος καλουμαζόμενον, ἐν τῇς τ ἐφεξῆς φθόγγων ἀκολουθίας τὴν τῆς ἁρμονίας ποιότητα φανερὸν γίνεσθαι συμβαίνει. Ariſt. Quint. p. 18. v. 7.

ſtrings hypate meſon and nete diezeugmenon fall out ſo, as to give the proper ſpecies for every mode; which circumſtance will not attend any other boundaries, common or varying, without altering the poſitions of the meſe, and thereby diſturbing the harmonic doctrine. But we have from Ptolemey ſufficient teſtimony to this point. In the eleventh chapter of his ſecond book, of which I have given a tranſlation above, we ſee, that the diapaſon in queſtion is choſen by him, to exhibit the diviſions, that belong to the ſeveral ſpecies. 'Tis true, he ſeems to inſinuate, that any other diapaſon might have been choſen, were it not for the difficulty the voice finds in running to the extremes: and in this, as a theoriſt, he is right; for muſical relations and proportions, conſidered as objects of mere ſpeculation, may be conceived at any pitch; but in the caſe before us, it was the practice of the inſtrument, that governed the theory. The pitch of the lyre had been made to anſwer to the double octave of the human voice; the pitch of the ſyſtem, in the Dorian mode, anſwered to that of the lyre; and, in every other mode, the difference in pitch from that of the Dorian was determined. Now, under ſuch limitations, that took their riſe from practical muſic, it is evident, that no other diapaſon, but that inſtanced by Ptolemey, could have ſerved his purpoſe.

But that the boundary of the ſpecies is rightly adjuſted in the diagram, there is yet another ſtrong teſtimony, from the tables of Ptolemey, annexed to the fifteenth chapter of book ſecond. To make this proof evident, it will be neceſſary to give ſome account of this part of his work.

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In the twelfth chapter of book ſecond, he begins to treat of the ſections of the harmonic canon, for proving, by experiment, the truth of the ratios he had delivered for the genera, and propoſes to exhibit theſe ſections for each genus, in all the ſeven tones. Then, after ſhewing the defects of the monochord canon in uſe, and (chapter thirteenth) the inſufficiency of the improvements made thereon by Didymus, he propoſes, and explains, his own method of applying the canon to eight ſtrings tuned in uniſon. But, before he proceeds to give the ſections upon all the tones, he thinks it neceſſary to ſhew, by experiment, the advantage of his own numbers, above thoſe of preceding harmonicians; for which purpoſe, the expoſition of them in one tone, that is, in the immutable ſyſtem, was ſufficient. Accordingly, in chapter fourteenth, (the greateſt part of which has been loſt, and is ſupplied from conjecture by Wallis) and in the tables annexed, he gives the ſections of the Dorian ſpecies of diapaſon; where the diazeuctic tone, lying in the middle, left a complete tetrachord on each ſide, for the expoſition of the generic numbers; and compares his own ratios with thoſe of Archytas, Eratoſthenes, Ariſtoxenus, and Didymus. Then, in the fifteenth chapter, he proceeds, as he propoſed, to give the ſections according to his own ratios, for the genera moſt in uſe in all the ſeven tones: theſe ſections he has included in two ſets of tables, each ſet containing ſeven, *viz.* one for each mode; and each table being again ſubdivided into five, for the generic differences. Now, of theſe two ſets of tables, the firſt juſtifies the diagram; for here he has given his numbers upon the eight ſtrings, from nete diezeug-

menon to hypate meſon; ſo that the diagram and the tables agree in every reſpect, except the generic differences, which I had no occaſion to conſider. In the ſecond ſet, indeed, the numbers are applied to the diapaſon, from meſe to proſlambanomenos; and there being no explanation in this chapter of the diſtinct uſe of this ſecond ſet, nor any reaſon aſſigned for giving it, but the convenience of having the option, to begin the tuning from nete or meſe (26), this may ſeem to furniſh an objection to the proof propoſed to be gained from theſe tables: but if we look forward to the ſecond chapter of his third book, we ſhall there ſee his meaning; which I ſhall proceed to explain.

Having, in the ſixteenth chapter, ſhewn how the numbers, given in the two ſets of tables, are to be applied to an octachord canon, he propoſes, in the firſt chapter of the third book, to ſhew, how the diviſions for the fifteen ſounds of the whole diſdiapaſon ſyſtem might, if required, be found, according to the ſame ratios he had already aſſigned for the extent of a diapaſon. Now, for the doing this, two inconveniences were to be obviated. Firſt, If the inſtrument, on which this experiment was to be tried, was to receive an addition of ſeven ſtrings, to complete the diſdiapaſon, and theſe additional ſtrings were all to be in uniſon with the eight before applied, it would happen, that, in ſhifting the moveable magas, or bridge, to the ſections required, thoſe ſtrings, which were to exhibit the acute ſections of the tetra-

(26) Ἵν' ἔχωμεν ἀφ' ὁποτέρας ἂν τῶν ἀρχῶν περαιρώμεθα ποιεῖσθαι τὰς ἁρμογάς. Ptol. Harm. p. 174. v. ult.

chord

chord hyperbolæon, would have ſo little ſpace left between the magas and the extremity of the ſtring, that it would be difficult for them to yield a diſtinct ſound. And ſecondly, The canon muſt be crowded with theſe additional diviſions, which would alſo have its inconvenience. To remedy this, he propoſes ſeveral methods; the firſt of which is, to fit the inſtrument with fifteen ſtrings, of which the eight, that were to receive the diviſions from meſe to nete hyperbolæon, ſhould be in uniſon, at the pitch of meſe, and the other ſeven in uniſon, at the pitch of proſlambanomenos. Now, as the ſucceſſion from the ſtring proſlambanomenos to the ſtring meſe, though it varied with the modes, was yet, in the ſame mode, always the ſame with that from meſe to nete hyperbolæon, the difference of a diapaſon in pitch excepted, it is plain, that, under this adjuſtment, a canon divided for the one diapaſon would ſerve equally for the other, the difference in pitch being eſtabliſhed before in the open ſtrings. By this method, therefore, the diviſions for the acuter diapaſon would be as large as thoſe for the graver, and the canon needed not to be crowded with a greater number of diviſions, than had been uſed for the ſingle diapaſon, by reaſon of its double application. The other methods, which he propoſed, I need not go through the explanation of, farther than to remark, that, for thoſe, as well as for this, a freſh ſet of numbers was neceſſary, thoſe contrived for the ſucceſſion of the diapaſon, from hypate meſon to nete diezeugmenon, being no-ways applicable to a ſucceſſion, which began from proſlambanomenos or meſe. And this, it ſeems, was his reaſon for giving the ſecond ſet of tables in his fifteenth chapter,

chapter, as appears from his own words, in the ſecond chapter of the third book.

In general, for ſuch uſes, as comprehend a diapaſon only, it behoves us to employ, out of the numbers exhibited, thoſe which contain the ſection from nete diezeugmenon, that the melos may be taken in the middle tenſions; but for ſuch as comprehend the diſdiapaſon, thoſe exhibited from nete hyperbolæon, or meſe, that the tuning may be adjuſted at both extremities alike (27).

Here we ſee he fully explains his former meaning; and, in recommending his firſt ſet of tables for the uſes of a diapaſon, gives the ſame reaſon for the choice of that diapaſon in particular, as he aſſigns for it in the eleventh chapter of the ſecond book, *viz.* that the melos may poſſeſs the middle of the inſtrument. But it muſt be obſerved, that when the whole diſdiapaſon has been adjuſted by the ſecond ſet of tables, the tunings of the eight ſtrings, from nete diezeugmenon to hypate meſon, will come out, for every mode and genus, the very ſame, as if they had been tuned by the firſt ſet: ſo that no inference can poſſibly be drawn from thence, either that the author has varied as to his own meaning, or that the diagram, which I have given, does not correſpond with it.

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(27) Καθόλου δὲ προσακτέον, τὰς ἐπικειμένας ἀρμονικὰς τῷ μὴ τὸ διὰ πασῶν μόνον περιεχούσαις χρήσεσι, τὰς ἀπὸ τῆς νήτης τῶν διεζευγμένων ἐχούσας τὴν καταληψιν· ἵνα ἐν τῷ μέσαις τάσεσιν ἐπιλαμβάνηται τὸ μέλος· τῷ δὲ τὸ δὶς διὰ πασῶν, τὰς ἀπὸ τῆς νήτης τῶν ὑπερβολαίαν, ἢ τῆς μέσης, ἐπιζευγνυμένας· ἵνα κατ' ἀμφοτέρων τῶν ἄκρων ἡ ὁμοίων ἐφαρμόζεσθαι δύνηται. Ptol. Harm. p. 228. v. 15.

5. I come now to the fifth point, which has been the ſubject of much diſpute, and which might well afford an endleſs controverſy, whilſt the union of the two doctrines was not underſtood; for whatever reaſons, from probability or teſtimony, might be urged in favour of the muſical, the harmonic doctrine appearing to be ſtill better ſupported, and ſeeming to contradict the other, was ſure to ſuſpend the deciſion, at leaſt, if not gain one in its own favour. But the agreement between the two doctrines being ſhewn, and their views diſtinguiſhed, the teſtimony borne to the harmonic doctrine will no longer be in the way of the muſical, and we may ſafely allow whatever appears favourable to the latter, its full weight. I ſhall therefore offer, in ſupport of this point, the ſix following arguments.

Firſt, No one accuſtomed to hear and judge of the effects of muſic, can conceive, that a mere alteration in pitch of the ſame melody, though in the hands of the moſt artful maſter, can have ſo powerful an effect, to change the mode or ſtyle of compoſition, as an alteration of the melodious ſucceſſion: or, if this ſhould be aſſerted, it might be diſproved, by an appeal to the works of all the compoſers of eminence among the moderns; who having, in all the variety of their modulations, but one change, that truly anſwers to the muſical doctrine of the modes, *viz.* the change of the ſucceſſion of the ſame key, from the major third to the minor, have all reſerved this change for their pathetic paſſages. I ſhall give but one inſtance of it, from our countryman Purcell, who, in his *Mad Beſs*, has ſo happily introduced this change, upon the words, " Cold and hungry am I

" grown,"

" grown," that if the performer does juſtice to the compoſition, the hearer can ſcarce help fancying himſelf affected with the very ſenſations, with which the ſtarts of frenzy are there pictured.

2dly, If I have been right in aſſigning a greater antiquity to the muſical doctrine, than to the harmonic, the effects of the modes muſt, in earlier times at leaſt, be referred to the former. And this carries with it an argument for later times alſo; for the old principle of the modes could not be ſuddenly changed. And though it may be urged, from the introduction of the eight ſpurious modes, that the harmonic doctrine came, in time, to be conſidered as a principle independent of the other, this will only prove a corruption of the better doctrine, which may ſafely be admitted; though that the muſical doctrine was ever quite diſuſed, unleſs in very late times indeed, I much doubt, as ſome traces of it are found in almoſt all the writers come down to us.

3dly, Ptolemey's rejecting the eight modes, that wanted the ſupport of the muſical doctrine, is another proof. For, if the harmonic doctrine had been the more eſſential of the two, thoſe modes ought to have been preſerved. Nor does the admiſſion of ſix of them, by the Ariſtoxenians in earlier times, weaken the force of this argument: for though the rejecting them is concluſive, againſt the harmonic doctrine, as far as any weight is allowed to the opinion of the rejecter, the argument from their admiſſion will not conclude in its favour, till it be proved, that in the uſe the ſpecies of diapaſon afforded by theſe modes was not attended to, but only the order from the proſlambanomenos, in power of each, as the favourers of the

the harmonic doctrine ſeem to ſuppoſe: and this proof it would not be eaſy to obtain, ſince each of theſe modes had its ſpecies belonging to it, and was only exceptionable, becauſe it repeated the melody of one or other of the ſeven.

4thly, The denominations given by the Ariſtoxenians to five of the ſpurious modes, after the genuine ones, whoſe ſpecies they afforded, furniſh another ſtrong proof; for this could ſcarce have happened, if the pitch of the ſyſtem had been the principal diſtinction.

5thly, We find in Plutarch, Pliny, and other authors, the invention of particular modes aſcribed to particular muſicians; which may be accounted for, on the ſuppoſition, that the modes were ſo many different ſpecies of diapaſon; ſince it requires great art and ſkill to introduce agreeably melodies, to which the ear has not been accuſtomed: but the taking the ſame melody at a different pitch is a variety, for which the inventor would hardly have had his name ſo carefully delivered to poſterity.

But 6thly, there are paſſages, that ſtrongly confirm the preceding arguments. Ariſtoxenus ſpeaks of modes of the melopœi (28), by which I underſtand him to mean the ſpecies of diapaſon, in contradiſtinction to the modes of the harmonicians; for I ſuſpect the modes of melopœïa, mentioned by Ariſtides Quintilianus (29), and divided by him into three kinds, *viz.* nomic, which was netoides; dithyrambic, which was meſoides; and tragic, which

(28) Pag. 40. v. 21.
(29) Pag. 29. v. ult.

was hypatoides; and which are alſo mentioned by other writers, of a low age (30); theſe modes, I ſay, I ſuſpect to be founded on diſtinctions not in uſe ſo early. The uſe of the ſpecies of diapaſon in melopœïa is confirmed alſo by Bacchius, who, immediately after explaining them, expreſſes himſelf thus:

" So many, therefore, in the art of muſic, are the ſpecies of the conſonances, by which every melopœïa is formed (31)."

And alſo by Ariſtides Quintilianus, who concludes his doctrine of the ſpecies, with theſe words:

" And thus much ſuffices concerning ſyſtems, which the antients alſo ſtyled the ethic principles (32)."

For, by ſyſtems, we are here to underſtand ſyſtems in the general ſenſe, as defined and treated by the Ariſtoxenians, whom Ariſtides follows in this part of his work, and more particularly the three conſonant ſyſtems, whoſe ſpecies he had juſt been deſcribing.

The paſſage I have already cited from Plato, ſerves alſo ſtrongly to ſupport this point; for the terms, ſpecies, and figure, are there uſed in the ſtrict technic ſenſe, and not merely to expreſs variety, as appears from his ſpeaking, in the ſame paſſage, afterwards of the ſpecies of melos, the ſenſe of which cannot be miſtaken; and that he is ſpeaking of muſic, in

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(30) Martianus Capella, p. 189. v. 21. Manuel Bryennius in Oper. Walliſii, p. 503. v. 14.

(31) Τοσαῦτα μὲν οὖν ἐστι τῶν συμφωνιῶν τὰ εἴδη ἐν τῇ μουσικῇ τέχνῃ, δι' ὧν πᾶσα μελοποιία συνίσταται. Bacch. Sen. Introd. Art. Muſic. p. 19. v. 18.

(32) Περὶ μὲν οὖν συστημάτων, ἃ ἐκ ἀρχὰς οἱ παλαιοὶ τῶν ἐθνῶν ἐκάλουν, ἀρκείτω ταῦτα. Ariſtid. Quintil. p. 18. v. 12.

reſpect

reſpect to its effects, is manifeſt, from the whole paſſage: ſo that, though the modes are not directly mentioned, they are neceſſarily to be implied. But Ptolemey is ſtill more expreſs; for, in the paſſage cited above, from book ſecond, chapter ſeventh, he directly affirms, that the ethos, or character of the mode, depended on the muſical doctrine, and not on the harmonic; and is ſtill more explicit, to the ſame purpoſe, in the paſſage cited alſo from the ſixth chapter of the ſame book. Theſe two paſſages are ſo clear, that there is no evading their teſtimony, but by ſuppoſing the author to be ſingular in his opinion; which there is, indeed, room to think was his caſe, with reſpect to many of the muſicians, his contemporaries; for the pains he has taken to clear up and diſtinguiſh the two doctrines, is a ſufficient proof, that miſtakes had prevailed concerning them; but that what he has advanced is not repugnant to the doctrines held in earlier antiquity, has, I think, been amply ſhewn. I ſhall now cloſe theſe proofs with three remarkable paſſages from Plutarch's dialogue on muſic, which will all become intelligible from the explanation given of this ſubject, at the ſame time, that they will ſerve to confirm it. The firſt reſpectts the invention of the Mixolydian mode, which we ſhall ſee he treats as a ſpecies of diapaſon, telling us between what ſounds of the ſyſtem it lay, and in what part of the diapaſon the diazeuctic tone was ſituated; in both which circumſtances, the paſſage agrees with the preceding explanation, and with the firſt diagram.

" Lyſis informs us, that Lamprocles the Athenian, ſeeing, that it (the Mixolydian harmony) had not the

disjunction (diazeuctic tone), where almoſt every one imagined (meaning in the middle of the diapaſon, for ſo it was in the Dorian, which was moſt familiar to the Greeks), but at the acute, made the figure of it ſuch as that from parameſe to hypate meſon (33)."

The next paſſage informs us of a very remarkable circumſtance, *viz.* that, after the addition made to the lyre of the tetrachord hypaton, no uſe was made of thoſe ſtrings in the Dorian mode, though they were employed in the reſt. Speaking of the earlier times, and arguing, that the antients had omitted many things, rather from choice than ignorance;

" It is manifeſt (ſays he), that, in reſpect to the [tetrachord] hypaton, it was not through ignorance, that they abſtained, in their Dorian [compoſitions], from this tetrachord; for they employed it in the reſt of the tones, as plainly knowing it; but, for the ſake of preſerving the ethos, they left it out in the Dorian tone, as reſpecting the beauty thereof (34)."

From this paſſage it is evident, that the Dorian melody, which, in its proper character, according to the muſical doctrine, was terminated by hypate meſon, was in ſuch eſteem with the Greeks, from a long habit of hearing their beſt pieces compoſed in that ſucceſſion, and within that compaſs,

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(33) Λύσις ὴ (φησι) Λαμπρόκλεα † Ἀθηναῖον συνιδόντα ὅτι οὐκ ἐν ταὐτὸ ἔχει † διάζευξιν, ὅπερ ἡδὺν ἄπαντες ἤκουον, ἀλλ' ἐπὶ τὸ ὀξύ, τοιοῦτον αὐτῆς ἀφῃρῆσθαι τὸ σχῆμα οἷον τὸ ἀπὸ παραμέσης ἐπὶ ὑπάτην ὑπάτων. Plutarch. Dial. de Muſica.

(34) Δῆλον ὴ κὴ τ̃ περὶ ὑπάτων, ὅτι οὐ δι' ἄγνοιαν ἀπεῖχοντο ἐν τοῖς δωρίοις τ̃ τετραχόρδ τούτ, αὐτίκα ἐπὶ τ̃ λοιπῶν τόνων ἐχρῶντο, δηλονότι εἰδότες δι' ὴ τ̃ τ̃ ἤθους φυλακὴν ἀφέρων ἐπὶ τ̃ δωρείου τόνου, τιμῶντες τὸ καλὸν αὐτ̃. Ibidem.

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that the additional ſtrings could not be ventured upon in that mode, though in the reſt they were admitted.

The laſt paſſage is that, which I have mentioned above to have been cenſured by Meibomius: we ſhall ſee here, that the author is expreſly arguing for confining harmonic within its proper ſphere, and not ſuffering it to encroach on melopœïa, by engaging with the rules of propriety, in the uſe of the elements; ſo that it both juſtifies and explains the diſtinction I have made above, between the two ſciences, and turns the cenſure of Meibomius upon himſelf, who ſeems, indeed, without excuſe, unleſs we ſuppoſe him to mean only, that Plutarch, in giving the Ariſtoxenian diviſion, ought not to have ſunk any part of it: which remark would have been juſt, if Plutarch had given the diviſion as ſuch; but he does not name the ſchool, though he adopts their diſtribution of the ſcience as far as the ſix diviſions he approved; ſo that no inconſiſtence can be charged upon him. The latter part of this paſſage alſo confirms the diſtinction I have made between the two doctrines, and their uſe in the two ſciences; ſo that it aſſiſts moſt of the preceding arguments and proofs: and here I ſhall therefore reſt this point, which will ſcarce be thought to need farther ſupport.

For it is evident, that the immediate objects of harmonic are genera, *viz.* thoſe of the hermoſmenon, intervals, ſyſtems, ſounds, tones, and ſyſtematic mutations; and farther than this it cannot go: ſo that we ought not to expect it ſhould determine how far the poet, in his muſic, has properly aſſumed the Hypo-

Hypodorian tone, for inſtance, for the outſet; or the Mixolydian and Dorian, for the concluſion; or the Hypophrygian and Phrygian, for the middle; ſince the harmonic treatiſe does not extend ſo far, but has need of many others. For of the force of propriety it cannot judge, ſince neither the chromatic nor the enharmonic genus will ever contain a perfect force of propriety, ſuch as to diſcover the ethos of the compoſition, that being the buſineſs of the artiſt. And again, it is plain, that the voice [pitch] of the ſyſtem is to be diſtinguiſhed from the melopœïa wrought in the ſyſtem, the conſideration whereof does not belong to the harmonic treatiſe (35). Having now finiſhed my head of proofs, I come,

Laſtly, To conſider how far this ſubject has been underſtood by Meibomius, Wallis, and ſome few others that have wrote ſince; and in what reſpect their explanations differ from my own.

In reſpect to the opinion of Meibomius, Malcolm tells us, that that writer, in his notes on Ariſtides Quintilianus, affirms the differences of the modes,

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(35) Δῆλον γὰρ ὅτι ἡ μὲν ἁρμονικὴ γλωσσῶν τε τῶν ἡρμοσμένων καὶ διαστημάτων καὶ συστημάτων καὶ φθόγγων καὶ τόνων καὶ μεταβολῶν συστηματικῶν ἐστι γνωσικὴ, πορρωτέρω δ' οὐκέτι ταύτην προσελθεῖν δύναται· ὥστε οὐδὲ ζητεῖν ἀφ' ἡμῶν ταύτης τὸ διαγνῶναι δύναιτ' ἄν πότερον οἰκείως εἴληφεν ὁ ποιητὴς, ὅμοιον εἰπεῖν, ἐν μέσαις τὸν ὑποδώριον τόνον ἐπὶ τὴν ἀρχὴν, ἢ τὸν μιξολύδιόν τε καὶ δώριον ἐπὶ τὴν ἔκβασιν, ἢ τὸν ὑποφρύγιόν τε καὶ φρύγιον ἐπὶ τὴν μέσην. Οὐ γὰρ διατείνει ἡ ἁρμονικὴ πραγματεία πρὸς τὰ τοιαῦτα, προσδεῖται δ' πολλῶν ἑτέρων· τὴν γὰρ τῆς οἰκειότητος δύναμιν ἀγνοεῖ. Οὔτε γὰρ τὸ χρωματικὸν γένος, οὔτε τὸ ἐναρμόνιον ἔζει ποτὲ ἔχον τὴν τῆς οἰκειότητος δύναμιν τελείαν καθ' ἣν τὸ τῶν πεποιημένων μέλους ἦθος ἐπιφαίνεται, ἀλλὰ τοῦτο τοῦ τεχνίτου ἔργον. Φανερὸν δ' ὅτι ἑτέρα τοῦ συστήματος ἡ φωνὴ τοῦ ἐν τῷ συστήματι κατασκευαζομένης μελοποιΐας, περὶ ἧς νῦν ἔστι θεωρῆσαι τὴν ἁρμονικῆς πραγματείας. Plutarch. Dial. de Muſica.

upon which all the different effects depended, to have been only in the tenſion, or acuteneſs and gravity of the whole ſyſtem (36). And indeed, in the note on page 13. verſe 4. of that author (37), which, I preſume, Malcolm had under his eye, Meibomius clearly decides for the harmonic doctrine, as he does alſo in his note on page 2. verſe 1. of Euclid's Harmonic Introduction (38). But in this laſt note, he had juſt before told us, that tone was by the antients alſo called harmonia, and ſpecies of diapaſon (39). And, in the concluſion of his note on page 1. verſe 10. of the ſame author, he delivers himſelf more fully to the ſame purpoſe.

" The antients (ſays he) having conſidered ſeveral ſpecies of diapaſon in the biſdiapaſon, called theſe alſo harmonics. Whence we read Dorian, Phrygian harmony, in the beſt writers. The ſame were again called tones and tropes, or modes (40)."

In

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(36) Malcolm's Treatiſe on Muſic, p. 540.

(37) Hic autem locus oppidò notandus eſt contra recentiorum de tonorum effectibus opiniones, illorum enim varietates, *ſo*, *la*, acuminis atque gravitatis differentia veteres unanimi conſenſu definiunt—Acumen autem ac gravitatem nihil varietatis cantilenæ adferre contra omnem eruditam vetuſtatem, imo communem ſenſum exiſtimant. Meibom. in Ariſt. Quint. p. 219.

(38) Tonus ſeu modus eſt totius ſyſtematis harmonici, hoc eſt biſdiapaſon aut ſimpliciter diapaſon differentia; ut Phrygius tonus à Dorio nulla alia re differt, quam quod totum Phrygii ſyſtema acutius ſit toto Dorii ſyſtemate, tono, qui eſt in ratione ſuperoctava. Meibom. in Euclid. Introd. Harm. p. 46.

(39) Toni vocabulum quatuor modis accipitur;—hîc idem eſt quod modus; cum dicimus tonus, ſive modus Dorius, Lydius, Mixolydius; veteribus quoque harmonia adpellatur, et ſpecies diapaſon. Ibidem.

(40) Cum autem plures diapaſon ſpecies in biſdiapaſon antiqui ſpectarent, illas quoque adpellarunt harmonias. Unde Doria, Phry-

gia

In his note alſo on the paſſage I have cited above, from Ariſtides, page 18. verſe 11. he explains the expreſſion, quality of the harmony, to ſignify the ſpecies of diapaſon, or tone (41), which is the ſenſe I have put upon it. We ſee therefore, that though he haſtily aſcribes to the harmonic doctrine alone the effects, which I have ſuppoſed to ariſe only from the muſical, yet he clearly admits both the doctrines to be warranted from antiquity; and I am glad to have ſo far the ſupport of this learned critic's opinion: but of the connection between the two doctrines, as I have explained it, I ſee no trace in his notes; nor is it to be imagined, but that, if he had ſeen it, he would have enlarged upon it.

In reſpect to Dr. Wallis, though he had the advantage of the notes of Meibomius, who had cleared up ſo many difficulties, and had alſo taken under his own management the text of Ptolemey, the author, of all others, the moſt likely to have given him a thorough inſight into this ſubject, yet we find him not only defective in his explanations of it, but, contrary to his uſual accuracy, even in miſleading his readers by falſe doctrines. With reſpect to the muſical doctrine, if we may judge by his ſilence, he appears to have ſeen leſs of it than Meibomius; for, in the appendix to his edition of Ptolemey, wherein he

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gia harmonia, apud optimos autores legimus. Deinde eædem toni et tropi, ſeu modi ſunt adpellati. Meibom. in Euclid. Introduct. Harm. p. 42.

(41) Id eſt quæ ſit ſpecies octachordi, ſeu quis tonus; nam duobus modis uſurpatur vox ἁρμονία, uno pro genere enarmonio, altero pro tono, ſeu tropo, ſeu modo; quæ ſignificatio eſt huic loco propria. Meibom. in Ariſt. Quint. p. 230.

under-

undertakes to explain the harmonic elements, after treating of the ſpecies of diapaſon, he gives not the leaſt hint, that they had any relation to the modes, except by giving their denominations; and in doing this, he expreſſes himſelf (42) as if he thought theſe denominations rather aſſigned for ſome ſeparate reaſon, than on account of their connection with the modes; though, as he was going immediately to the doctrine of the tones, this was the place, where he might have been expected to have taken notice of the connection between the two doctrines, if he had obſerved it. In treating of the modes, indeed, though he explains them on the foot of the harmonic doctrine only (43), he aſſigns ſome of his author's reaſons for reducing them to ſeven (44). And his note alſo on the beginning

(42) Atque hæ quidem diapaſon ſpecies ſeptem ſua ſingulæ ſortiebantur nomina; prima dicta eſt Mixolydia; ſecunda, Lydia; tertia, Phrygia; quarta, Doria; quinta, Hypolydia; ſexta, Hypophrygia; ſeptima, tum Locrenſis tum Hypodoria. Tandem de modis, ſeu tonis dicendum reſtat. App. ad Ptol. p. 311.

(43) Modus itaque, ſeu tonus, prout hic ſumitur, denotat vocis locum, non quo una vox, ſed quo tota vocum ſeries, ſeu ſyſtema canitur; acutiorem puta, gravioremve. Utpote prout apud nos *mi* canitur, nunc in *b f b mi*, nunc in *elami*, nunc in *alamire*, &c: Sic apud illos verbi gratia, parameſe poteſtate (quod tantumdem eſt atque noſtrum *mi*), poſita erat nunc in parameſe poſitione, nunc in poſitione nete diezeugmenon, nunc in meſe, &c. Ibid. p. 312.

(44) Contra hos, qui tonos, ſeu modos, ſic augent per hemitonia diſputat Ptolemæus, cap. 7, 8, 9, 10, 11. lib. ii. docetque modorum variorum uſum non in hunc ſolum finem introductum, ut acutior graviorve ſit totius cantus tenor; quippe huic ſufficeret cantoris vox acutior graviorve; aut muſici organi ad hoſce tenores accommodatio.—Sed eo potiſſimum fine fuiſſe introductum, ut in ipſius cantus curriculo tranſitus fiat de modo in modum, quam vo-

ginning of chapter eleventh, book ſecond (45), rightly explains the ſpecies of diapaſon, as they lay between hypate meſon and nete diezeugmenon: but this interpretation is formally introduced with a *nempe hoc vult*, as if he thought his author ſingular in this doctrine, and looked upon the doctrine itſelf, as ſtated in that paſſage, rather as explanatory of an incidental circumſtance attending the harmonic doctrine, than meant to aſſign the true doctrine of the modes. This is manifeſt, from his drawing the ſame inference, both in this note and elſewhere (46), from this muſical doctrine of the ſpecies, as he had drawn before from the harmonic one, *viz.* that the tones thus

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cant (μεταβολην κατα τονον) mutationem ſecundum tonum: quod à noſtris fit mutata clavis ſignatura adhibitis mollis et duri notis, &c. —Et propterea tonos diſtinctos ponendos eſſe docet omnino ſeptem, Mixolydium, Lydium, Phrygium, Dorium, Hypolydium, Hypophrygium, et Hypodorium. Totidemque admittit hodierna muſica pro varia clavis ſignatura. Ibid. p. 313.

(45) Nempe hoc vult: diapaſon illud expoſitum ab hypate meſon ad neten diezeugmenon; hoc eſt (in muſica hodierna), ab elami ad elami, his vocibus poſt primam cani in tono Mixolydio, *fa, ſol, la, fa, ſol, la, mi*; quæ eſt prima ſpecies diapaſon, habens *mi*, ſeu tonum diazeuctícum in loco præcedente, ſeu acutiſſimo: his in Lydio, *ſol, la, fa, ſol, la, mi, fa*; quæ eſt ſpecies ſecunda diapaſon, habens *mi*, in loco ſecundo ab acutiſſimo: in Phrygio, *la, fa, ſol, la, mi, fa, ſol:* in Dorio, *fa, ſol, la, mi, fa, ſol, la:* in Hypolydio, *ſol, la, mi, fa, ſol, la, fa:* in Hypophrygio, *la, mi, fa, ſol, la, fa, ſol:* in Hypodorio, *mi, fa, ſol, la, fa, ſol, la*; quæ ſunt tertia, quarta, quinta, ſexta, et ſeptima ſpecies diapaſon; habentes *mi*, in loco tertio, quarto, quinto, ſexto, et ſeptimo, ab acutiſſimo: quæ omnia in muſica noſtra mollis et duri notis varie poſitis indicantur, ut mox dicemus. Not. ad Ptol. p. 136.

(46) Ut ſoli ſuperſint Ptolemæi ſeptem, Hypodorius, Hypophrygius, &c. totidemque agnoſcit hodierna muſica poſito *mi* in F, G, A, *b*, *c*, *d*, *e*. Ibid. p. 154, ad finem.

ſettled

ſettled by Ptolemey are acknowleged by the moderns, in their ſeven poſitions of the *mi*. For nothing ſavours leſs of the muſical doctrine, than what the moderns hold in this reſpect. And it is manifeſt alſo, from his neglecting to aſſiſt the explanation of this doctrine of Ptolemey, by any citations from other authors, that ſpeak to the ſame purpoſe; which he would not have failed to do, if it had ſtruck him as an antient and genuine theory of the modes. But it is plain to me, that, however he may have penetrated the ſenſe of theſe particular paſſages, he did not graſp their conſequences. This the miſtakes he has fallen into ſufficiently ſhew; which I ſhall now proceed to point out, and which a thorough comprehenſion of the ſubject could not but have prevented.

In ſpeaking of the meſe of the Hypophrygian mode, he explains it by $c\#$, inſtead of $f\#$ (47), a miſtake, which, I think, muſt be imputed to his haſte.

In the table of the modes, which he has given from Meibomius (48), he has placed the letters in uſe with the moderns, againſt the Hypodorian, as well as the Dorian, making A anſwer to the proſlambanomenos of the former, as well as the latter; by which, it is to be preſumed, he means them to expreſs the abſolute pitch of the ſounds in the Dorian, and in the Hypodorian, to denote only the general relations of the ſyſtem; but this ſhould have been farther explained.

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(47) Not. ad Ptol. p. 154. lin. 41.
(48) P. 155.

But his greateſt miſtake, and which I know not how to reconcile to his uſual caution, is in the method he gives for finding the places of the meſes for the ſeven modes. Not only is this method erroneous, but he gives it expreſly as the method of Ptolemey (49), though no ſuch direction is to be found in his work. As I have already given Ptolemey's method, in our modern terms, for the ſake of brevity, from his tenth chapter, where it is delivered, I ſhall here give that of Wallis, in the ſame terms, that the difference between the two may more eaſily be ſeen.

" Firſt pitch the Dorian, which is the middle tone, ſuppoſe in A; riſe a fourth to D, for the Mixolydian; fall a fifth to G, for the Hypolydian; riſe a fourth to C, for the Lydian. Then begin from the Dorian again, and fall a fourth to E, for the Hypodorian; riſe a fifth to B, for the Phrygian; and fall a fourth to F#, for the Hypophrygian (50)."

By this method, we ſee the meſes of the Lydian and Hypolydian are brought out at *c* and *f* natural; whereas, by Ptolemey's, they come out at *c*# and *f*#, where I have placed them.

This miſtake, I apprehend, the doctor was led into by the eleventh chapter of Ptolemey's ſecond book, where the meſes of the Lydian and Hypolydian are ſettled in trite diezeugmenon and lichanos meſon; which ſtrings, in their natural ſituation, in the Dorian mode, were tuned to *c* and *g* natural;

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(49) Hanc autem methodum adhibet Ptolemæus in tonis ſuis ſeptem deſignandis, &c. — primum omnium facit, &c. ſecundo tonum ſumit, &c. App. ad Ptol. p. 313 & 314.

(50) Ibid. p. 313. lin. ult. uſque ad p. 315. lin. 20.

but,

but, in this chapter, the author means only to fix the numerical ſtring allotted for each meſe, without regard to its pitch, which was to be regulated by the diſtances aſſigned for the tones in the tenth chapter. And by theſe diſtances, and all other concurring circumſtances, it is manifeſt, that the two ſtrings in queſtion were, in theſe modes, to be made a ſemitone, more acute than their natural ſituation, as I have already obſerved, in explaining the harmonic doctrine. How, therefore, the doctor could ſo far wander from the true theory, which lay before him in the text of his author's tenth chapter, as to ſubſtitute a different method, and deliver it, with great prolixity of explanation, as the method of his author, is what I know not how to account for, much leſs reconcile to his accuſtomed care and fidelity. What is ſtill more extraordinary, is, that, after finiſhing the directions and explanations of his own method, he, in the very next paragraph, cites the very tenth chapter of Ptolemey, where the method is given (51), in order to infer with his author, that the Mixolydian tone was diſtant from the Lydian a hemitone; the Lydian from the Phrygian, a tone, *&c.* though this inference, which is true only from Ptolemey's method, directly contradicts all he had been delivering. Could he overlook the falſeneſs of this inference, whilſt he was taking ſo much pains

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(51) His poſitis, inde colligitur (ut cap. 10.) toni Mixolydii à Lydio diſtantiam limma, ſeu craſſius loquendo hemitonium; hujus à Phrygio, tonum; hujus à Dorio, tonum; Dorii ab Hypolydio limma; Hypolydii ab Hypophrygio, tonum; hujuſque ab Hypodorio, itidem tonum. App. p. 315. lin. 21.

with

with the ſubject? But not to inſiſt farther on the ſlips of a writer, to whom the learned world ſtands ſo highly indebted, I ſhall take leave of him, with this remark only, that whatever he may have ſeen of the truth of theſe doctrines himſelf, his explanations have not ſucceeded in making the ſubject clear to ſucceeding writers; thoſe I have ſeen having either adopted the harmonic doctrine only, or been ſo confounded between the two, as to give a right account of neither. This has, in particular, been the caſe with Malcolm, who, in his Treatiſe on Muſic, explains that of the antients, and has taken ſome pains to reconcile the two doctrines of the modes. The greateſt part of what this writer delivers is not only falſe, in reſpect to the order, poſitions, and, indeed, almoſt every other circumſtance attending the modes, but, at the ſame time, ſcarce intelligible; or, if any meaning can be put upon it, it is too foreign to the truth of either of the doctrines, to be worth conſidering. I ſhall content myſelf with citing a paſſage from him, where his reaſoning is the cleareſt, and where we may ſee, that, after all the pains he has taken to reconcile the two doctrines, he owns himſelf unable to make any ſure deciſion upon the ſubject.

" He (Ptolemey) ſays, in the beginning of that chapter (cap. 7. lib. ii.), the mutations, which are made by whole ſyſtems (which we properly call tones, becauſe theſe differences conſiſt in tenſion), are infinite, with reſpect to poſſibility, as ſounds are; but actually, and with reſpect to ſenſe, they are finite. All this ſeems plainly to put the difference of the tones only in the acuteneſs or gravity of the whole; elſe, how do their differences conſiſt in tenſion, which ſignifies

ſignifies a certain tenor or degree of tune? and how can they be called infinite, if they depend on the conſtitution of the octave? Yet, elſewhere, he argues, that they are no other than the ſpecies of octaves, and as ſuch, makes their number ſeven; and accordingly, in all his ſchemes, ſets down their different modulations. But, in chapter ſixth, he ſeems more plainly to take in both theſe differences; for he ſays, there are two principal differences with reſpect to the change of the tone, one, whereby the whole ſong is ſung higher or lower, the other wherein there is a change of the melody to another ſpecies than it was begun in; but this, he thinks, is rather a change of the ſong, or melos, than of the tone; as if again he would have us think, this depended only on the acuteneſs and gravity of the whole. So obſcurely has the beſt of all the antient writers delivered himſelf on this article, that deſerved to have been moſt clearly handled. But, that I may have done with it, I ſhall only ſay, it muſt be taken in one or other of the ſenſes mentioned, if not in both; for another, I think, cannot be found (52)."

Notwithſtanding the confuſion in this author's explanations, and his leaving the queſtion thus undecided, it is his account of the modes, that ſeems chiefly to have been adopted by compilers ſince. And hence we find nothing better in the Cyclopædia of Chambers, or the Muſical Dictionary of Graſſineau, than a repetition of theſe doubts and perplexities. Amongſt the French writers, Broſſard, in his *Dictionaire de Muſique*, throws no light upon this ſub-

(52) Malcolm's Treatiſe on Muſic, p. 538. lin. ult.

ject,

ject, his account of the modes reſpecting chiefly thoſe of much later times, which were diſtinguiſhed into plagal and authentic, and with which the preſent queſtion has very little concern. Nor have I met with any thing ſatisfactory from their academicians, though ſome of them appear to have taken great pains with the ſubject. The writer amongſt theſe, who ſeems the moſt converſant with the muſic of the anantients, is Monſieur Burette, who, in the Memoirs of literature, has furniſhed many pieces on the various branches of the ſcience. Amongſt others, there is a diſſertation of his on the melopœïa of the antient muſic, which the learned academician ſhould rather have intituled a diſſertation on harmonic; for it contains an explanation of the elements of that ſcience. This author ſeems little apprized of the muſical doctrine; nor has he given a right account even of the harmonic one, having followed Wallis in his miſtake, in reſpect to the method, by which Ptolemey fixed the poſitions of his ſeven modes (53), and even drawn the ſame contradictory concluſion (54):

(53) Qu'ainſi on devoit renfermer dans l'eſpace d'une octave tous les modes, dont le Dorien devoit occuper, comme le centre, les ſix autres etant diſpoſez de façon, que le Mixolydien fut d'une quarte plus haut que le Dorien; l'Hypolydien d'une quinte plus bas que le Mixolydien; le Lydien d'une quarte plus haut que l'Hypolidien; l'Hypodorien d'une quarte plus bas que le Dorien; le Phrygien d'une quinte plus haut que l'Hypodorien; et l'Hypophrygien d'une quarte plus bas que le Phrygien. Diſſertation ſur le Melopée, &c.

(54) D'ou il paroit, qu'a compter de l'Hypodorien, qui eſt le mode le plus bas, il y à de celui ci à l'Hypophrygien l'intervalle d'un ton; de l'Hypophrygien à l'Hypolydien un autre ton; de l'Hypolydien au Dorien, un demiton; de ce dernier au Phrygien, un ton; du Phrygien au Lydien encore un ton; et du Lydien au Mixolydien, un demiton. Ibidem.

whence

whence it appears, that he relied on the doctor's appendix for this part at leaſt of his explanations. In his notes on the dialogue of Plutarch, printed alſo in the ſame memoirs, the not underſtanding the muſical doctrine has led him into a very falſe explanation of the paſſage cited above, concerning the invention of the Mixolydian mode. He ſets out (55) with excluding the only ſuppoſition, that could lead him to the ſenſe of his author, by denying, that the octachord ſyſtem could have any thing to do with this invention; and then flies (56) to two other ſuppoſitions, *viz.* that either the hendecachord or diſdiapaſon was here meant, both which are equally wide of the truth. The public is much indebted to this writer, for the laborious collections he has made in theſe notes, concerning the antient muſicians; but his harmonic explanations are not always to be relied on.

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(55) Lamprocle n'eſt point ici donné comme l'auteur de l'harmonie Mixolydienne, il en eſt regardé ſeulement comme le reformateur. Mais en quoi pouvoit conſiſter cette reforme? C'etoit, comme le dit Plutarque, à determiner le veritable ſyſteme de cette harmonie, ou de ce mode, quant à ſa disjonction, ou à l'arrangement des divers tetrachordes, qui compoſoient ce ſyſteme. En le reduiſant à l'etendue de l'octave, ou de l'octachorde, c'eſt à dire du double tetrachorde disjoint; le lieu de cette disjonction eſt unique, et par conſequent n'eſt point equivoque, comme on le peut voir par la progreſſion de ce ſyſteme, que voici, *mi*, *fa*, *ſol*, *la*, *ſi*, *ut*, *re*, *mi*; il ne s'agit donc point ici du double tetrachorde disjoint. Note 114.

(56) Mais l'hendecachorde, ou le triple tetrachorde disjoint, pouvoit etre le ſyſteme, dont parle icy Plutarque—ſuppoſé qu'il ſoit icy queſtion du ſyſteme complet de l'ancienne muſique, &c. Ibidem.

The laſt writer, that I have ſeen, who has treated this ſubject, and with whoſe opinion I ſhall conclude theſe ſheets, is the anonymous author of a letter to Mr. Aviſon, concerning the muſic of the antients, publiſhed with the eſſay of the latter on muſical expreſſion, in 1753. This author, who profeſſes but a ſlender knowlege of the theory, either of modern or antient muſic, has, in a few pages, diſcovered himſelf to be poſſeſſed of more than his modeſty will permit him to lay claim to. In the matter of the tones, however, (ſuch is the fatality, that ſeems to have conſtantly attended this doctrine) he does not appear to have ſucceeded better than the writers that have gone before him. He blames Sanadon and Cerceau, for affirming, in their obſervations on Horace, carm. 5. 9. that the Dorian mode anſwered exactly to our A-mi-la with a minor third, and the Phrygian to our A-mi-la with a major third. Now, that theſe French critics, though right, inaſmuch as they ſeem to conſider the modes as different ſpecies of diapaſon, are, nevertheleſs, miſtaken in their application of them, does, indeed, appear from my firſt diagram, where it may be immediately ſeen, that what they aſſert of the Dorian and Phrygian modes, is true only of the Hypodorian and Lydian; and had the anonymous writer gone no farther in his cenſure of them, I ſhould readily have joined with him: but he rejects the muſical doctrine entirely, and admits only of the harmonic. "Surely (ſays he (57), ſpeaking of what theſe critics advance) this is a muſical error, and a dream from the ivory gate. Two modes

(57) Page 6.

with

with the ſame tonic note, the one neither acuter nor graver than the other, make no part of the old ſyſtem of modes." And, agreeably to this opinion, he had before (58) deſcribed the ſeven modes, as reducible to one mode, taken higher or lower; which, he ſays, may be be called the mode of A, and muſt have had a minor third, as *c* natural is a minor third to A-mi-la. Whether this be a juſt account of the nature of the modes, I muſt now ſubmit, having ſaid already what has occurred to me, in ſupport of the contrary opinion.

To conclude, if what has been ſaid ſhould be thought to eſtabliſh any certainty concerning this matter, I ſhould hope it might tend to revive an inquiry into the muſic of the antients, which ſeems to have ſuffered an interruption from the difficulty of arriving at a right underſtanding of this material branch of it. Such an inquiry may, perhaps, be judged well worth purſuing, not only from the advantages, which modern muſic might derive from it, but alſo for the ſake of improving the art of poetry, and of better underſtanding and taſting the noble remains we have of the antient poets; the greateſt beauties of whoſe works, eſpecially the lyric and dramatic, are probably loſt to us, for want of ſeeing their connection with this ſcience.

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(58) Page 5.

LXIX. *An Enquiry into the Meaſure of the* Roman *Foot*; *by* Matthew Raper, *Eſq*; *F. R. S.*

Read May 15, 22, and June 5, 1760.

THE methods that have hitherto been taken to diſcover the meaſure of the Roman foot, will, upon examination, be found ſo unſatisfactory, that it is no wonder the learned are not yet agreed in that point.

The antient foot-rules now remaining; the repreſentations of the foot in ſculpture; and the meaſure of it, derived from the congius, differ ſo much among themſelves, and from each other, as to be inſufficient evidences ſeparately: and the great diſagreement of the foot from the congius, with the reſt, has not hitherto been ſatisfactorily accounted for.

The foot-rules found in old ruins at Rome, are of various lengths; and the age of none of them being certainly known, no preciſe meaſure can be determined from them, otherwiſe than by taking a mean from ſuch as appear to be moſt perfect. But though this may have been the foot in uſe at ſome time or other, yet as theſe rules are probably of different ages, both the greateſt and the leaſt of them may have anſwered to the ſtandards of their times.

For though we have no account of any alteration ever made in the ſtandard of the Roman foot, yet the wear of a ſtandard meaſure by uſe, and the making new to replace the old ones, muſt, in all probability, create a difference; eſpecially, as the Romans had not thoſe inducements to ſo preciſe an accuracy

accuracy in theſe matters, as the later diſcoveries in natural philoſophy (particularly the invention of the pendulum) have introduced among the moderns. Add to this, that the different ſtate of Europe, which has, for ſome ages, been divided into many conſiderable kingdoms and ſovereign ſtates, independent on each other, equally civilized, and carrying on a more conſtant and regular mercantile commerce with each other, than was known to the Romans, muſt neceſſarily introduce more frequent enquiries into the weights and meaſures of the different ſtates, and a more careful examination of the reſpective ſtandards of each, than the Romans could have any occaſion for: and uſe in theſe matters, is the parent of accuracy.

We can arrive at no greater certainty from the marbles, than from the foot-rules. Theſe, indeed, do not differ ſo widely from each other, as the rules; which ſeems to be the reaſon why moſt of the writers on this ſubject have given them the preference: but of the four that are extant, no two agree in the ſame meaſure; nor is the age of any one of them known: and as they were intended for repreſentations only, and not for uſe, their accuracy may reaſonably be doubted.

Feſtus, Frontinus, and Rhemnius Fannius, ſay, the ſide of the quadrantal (which contained 8 congii) was a Roman foot (1). A ſtandard congius of Veſpaſian

(1) Quadrantal vocabant antiqui quam ex Græco ἀμφορέα dicunt, quod vas pedis quadrati octo et quadraginta capit ſextarios. Feſtus in v. Quadrantal. Pes quadratus concavus capit amphora trimodia

Veſpaſian is ſtill in being, and has been meaſured by ſeveral learned men; but the foot derived from it exceeds thoſe on the marbles, and the greateſt number of the foot-rules, ſo much, that Mr. Greaves could find no better way of accounting for ſo great a difference, than by ſuppoſing what Feſtus and Fannius ſay (for he does not quote Frontinus), to be a vulgar error (2): whereas the name of this ſtandard ſhews its figure to have been a cube (3); which adds probability to their teſtimony, that its ſide was intended for the meaſure of the foot.

The meaſures of public roads in the itineraries, can be of little uſe in this enquiry; for they omit fractions, and we do not know whether the diſtances of the towns are reckoned from the market-places or from the gates; but a difference of half a mile in ſixty, is equivalent to the tenth part of an inch in the foot: therefore, no exact meaſure is to be expected from thence, even though the modern menſurations of Caſſini, Riccioli, and others, were more unexceptionable than they really are.

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trimodia [read, amphoram trimodiam]. Frontinus, in Expoſitione Formarum. Though the paſſage is corrupt, there can be no doubt of the meaning.

Pes longo ſpatio latoque notetur in anglo,
Angulus ut par ſit quem claudit linea triplex;
Quattuor ex quadris medium cingatur inane,
Amphora ſit cubus: quem ne violare liceret,
Sacravere Jovi, Tarpeio in monte, Quirites.

Rh. Fannius, de Pond. et Menſ.

(2) See Dr. Birch's edition of Greaves's Works, p. 228.

(3) Quæ illi κύβους, nos quadrantalia dicimus. A. Gellius, l. i. c. 20.

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The diſtances between the antient mile-ſtones are not liable to theſe objections; and if a ſufficient number of ſuch as ſtand neareſt to Rome were carefully meaſured, their authority would be conſiderable. But I do not find that any are now ſtanding within thirty miles of that city, nor that any of theſe have been meaſured, or even any in Italy; and provincial meaſures are not of equal authority.

There is ſtill another method, whereby we may diſcover the meaſure of the Roman foot; which is, from the remains of the antient buildings now ſtanding at Rome: and though many have made uſe of ſome ſingle parts of them for this purpoſe, yet no one hath hitherto compared the meaſures of the principal parts of any one of them with each other, which is the only way to diſcover the meaſure whereby a building was conſtructed.

With this view, therefore, I ſhall carefully examine the meaſures of the buildings contained in that ineſtimable treaſury of antient Roman architecture, intituled, Les Edifices antiques de Rome, and publiſhed at Paris, by Monſ. Deſgodetz, in the year 1682.

In order to this, it will firſt be neceſſary to aſcertain the proportion of the Paris foot (the meaſure uſed by this author) to ſome known Engliſh ſtandard.

The Paris foot is one ſixth part of the toiſe in the Chatelet; which was renewed in the year 1668 (4), and the new ſtandard has continued in uſe ever ſince.

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(4) See De la Hire's Diſſertation on the Roman Foot, in the Memoirs of the Academy of Sciences for the year 1714; where he gives an account of this renewal of the ſtandard in the Chatelet.

In

In the year 1742 (5), the Royal Academy of Sciences at Paris, at the requeſt of the Royal Society of London, ſent over a meaſure of half the toiſe of the Chatelet; from which Mr. Graham determined the proportion of the Paris foot to that of London, to be as 1065,41⅔ to 1000. Monſ. Le Monnier, of the Royal Academy of Sciences, from the ſame originals, found their proportion as 864 to 811, or as 1065,351 to 1000. The difference is inconſiderable, and we may, without injuſtice to Mr. Graham's known ſkill and accuracy in theſe matters, ſuppoſe their true proportion to be as 1065,4 to 1000.

Mr. Graham's meaſure of the London yard, together with that of half the toiſe of the Chatelet, are depoſited in the archives of the Royal Society at London, and of the Royal Academy of Sciences at Paris; and whenever I ſhall mention the London foot, without ſpecifying any particular ſtandard of it, I would be underſtood to mean this meaſure.

In this enquiry, we are to ſeek a common meaſure to the ſeveral parts of each building, that ſhall not differ very widely from ſome aſſumed magnitude of the Roman foot: and though we might take this aſſumption from any of the antient foot-rules now remaining, yet the nearer it is taken to the truth, the better guide it will be to us, and the more it will facilitate our enquiry. Now, as a mean meaſure, derived from theſe rules, will, probably, be nearer the truth, than either the greateſt or the leaſt of them, ſo one that ſhall include ſuch other remains of antiquity, as have hitherto been made of to diſcover

(5) See Philoſ. Tranſ. N° 465.

the

the meaſure of the Roman foot, will be ſtill more unexceptionable, as the writers on this ſubject are not agreed, which of the different authorities is to be preferred.

The repreſentations of this foot in ſculpture, are four in number; one on the ſepulchral monument of Coſſutius, formerly in the Colotian gardens at Rome; another on that of Statilius, in the Belvedere; a third on that of M. Ebutius, in the Villa Mattei (6); and the fourth on a marble, without inſcription, dug up of late years in the Via Aurelia, which, being in the poſſeſſion of the Marquis Capponi, is called, by P. Revillas, the Capponian foot.

Moſt of the early writers on this ſubject have expreſſed their meaſure of the Roman foot by a diagram; and Snellius (7) obſerving, that the paper contracted in drying, after the impreſſion was taken off, endeavoured to make a proper allowance for it. But Greaves, finding the meaſures of theſe figures to differ in different copies of the ſame impreſſion, took another method; and ſeems to have been the firſt that compared the original figures on the monuments of Coſſutius and Statilius with a modern ſtandard. This he did with ſuch care and diligence, that his meaſures deſerve a particular examination.

The London foot, which he uſed upon this occaſion, was taken from the iron ſtandard of 3 feet in

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(6) Picard and Auzout (in a paper called De Menſuris, hereafter quoted) have given the meaſure of the foot on this monument of Ebutius for the Colotian foot.

(7) See his Diſſertation on the Roman Foot, in the 3d vol. of Saggi di Diſſertazioni Academiche di Cortona.

the Guild-hall, London (8); which having been long ſince loſt, or deſtroyed, we have nothing left whereby to diſcover its true magnitude, but the meaſures others have taken of it, and thoſe which have ſince been taken of ſuch magnitudes as Greaves had compared with his copy of it.

Snellius, from a meaſure ſent him of this iron ſtandard (9), determined the proportion of the Rhynland to the London foot, as 1000 to 968. The Rhynland foot, according to Picard (1), contains 696 ſuch parts as the Paris foot contains 720: whence the proportion of the latter to this meaſure from the iron ſtandard, is as 1065,4 to 997 nearly. Eiſenſchmid found the Rhynland foot to contain 1391,3 ſuch parts as the Paris foot contains 1440 (2); which gives 1065,4 to leſs than 996½, for the proportion of the Paris foot to that of the iron ſtandard. Huyghens makes the Paris to the Rhynland foot as 144 to 139 (3); whence the proportion of the former to Snellius's London foot, will be nearly as 1065,4 to 995½. But there is reaſon to believe, that Huyghens's meaſure of the Rhynland foot was too ſmall (4).

By

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(8) Greaves, p. 223.

(9) Eratoſthenes Batavus, p. 125. 141.

(1) See a paper intituled De Menſuris, in Divers Ouvrages de Mathematique et de Phyſique, par Meſſrs. de l'Academie Royale des Sciences, Paris 1693, in folio, and afterwards printed in the 4th vol. of Ouvrages adoptez.

(2) Eiſenſchmid de Pond. et Menſ. vett. p. 94.

(3) Horolog. Oſcill. part iv. prop. 25.

(4) Picard, in his Voyage d'Uranibourg, p. 64. edit. Amſt. ſays, " Paſſant par Holland je pris l'occaſion de verifier la proportion

By theſe compariſons it appears, that Snellius's meaſure of the London foot, from this iron ſtandard, was at leaſt 3 parts in 1000 ſhorter than Graham's London foot.

Our countryman Norwood, in the year 1635, meaſured the diſtance between London and York, in order to determine the length of a degree on the meridian; which he found to contain 367196 London feet of this iron ſtandard (5). The French found the meaſure of a degree, in the latitude 66° 20′, to be 57438 toiſes, and at the equator 56783. Hence the meaſure of a degree in 52° 44′ (the middle latitude between London and York) will be found to be 57276 toiſes, or 343656 Paris feet. Theſe numbers give the proportion of the Paris foot to that of the iron ſtandard, as 1065,4 to 997,1 —, wanting ſomewhat leſs than 3 parts in 1000 of Graham's London foot.

Picard's paper De Menſuris, quoted above, and another on the ſame ſubject by Auzout, printed with it, contain ſome meaſures, which Greaves had before compared with his London foot. Both theſe papers were written after the renewal of the ſtandard of the Chatelet, in 1668. The former is ſo full of inaccuracies and miſtakes, that little uſe can be made of it; but Auzout's meaſures appear to be accurate; and as he ſeems to have taken his Paris foot from the

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portion du pied de Paris à celui du Rhin, dont l'original eſt à Leyde; laquelle proportion me parut eſtre exactement comme 720 à 696, au lieu de 720 à 695, que j'avois ſuppoſée dans la meſure de la terre." This latter is Huyghens's proportion.

(5) See Norwood's Seaman's Practice.

toiſe in the Chatelet for this purpoſe, it was probably a correct meaſure of that ſtandard.

Such of his meaſures as anſwer to Greaves's, are here reduced to thouſandth parts of the London foot, reckoning his Paris foot to contain 1065,4 ſuch parts.

	Greaves.	Auzout.	Diff.	Diff. *per* foot.
The Statilian foot	972.	969,96	— 2,04	2,1
The braccio of Florence	1913.	1908,84	— 4,16	2,18
The braccio of Siena	1974.	1973,21	— 0,79	0,4
Pætus's palm	732.	731,35	— 0,65	0,89

All theſe differences fall the ſame way, and ſhew, that Greaves's London foot bore a leſs proportion to Auzout's Paris foot, than that of 1000 to 1065,4. The Statilian foot, and the braccio of Florence, give the greateſt differences; one is carved in relievo, and the other is a rod; ſo that both are ſubject to diminution, by time and uſe. The palm is engraved; and Auzout, from the deviation of Pætus's Greek and Roman foot, from the common received proportion of 25 to 24, ſuſpected, that the latter was lengthened by wear; if ſo, the like muſt have happened to the palm. How their meaſures of the braccio of Siena come to differ ſo little, I cannot account for. But we can draw no certain concluſion from this compariſon, till we know whether any, and what, allowance is to be made for the diminution or increaſe of theſe magnitudes, in the interval between Greaves's and Auzout's meaſuring them.

For this purpoſe, I ſhall compare Auzout's meaſures, with thoſe taken by P. Revillas, with a correct

rect London foot (6), between fifty and ſixty years after. Theſe he has converted into Paris meaſure, by the proportion of 864 to 811; and I have reduced them back again, according to the ſame proportion.

	Auzout.	Revillas.	Diff.	Diff. *per* foot.
The Statilian foot	969,96	969,79	— 0,17	0,18 —
The Ebutian foot	972,91	972,22	— 0,69	0,71 —
Pætus's palm	731,35	732,65	+ 1,3	1,78
Pætus's foot	966,26	968,74	+ 2,48	2,57
	or 967 —		or 1,75	1,81

Hence the carvings appear to have ſuffered very little diminution in this interval; but Pætus's meaſures, which are engraved, and in conſtant uſe as public ſtandards, have been conſiderably lengthened. Therefore, allowing for both theſe circumſtances, we may conclude, that Greaves's London foot wanted 2 parts in 1000, to be to Auzout's Paris foot, in the proportion of 1000 to 1065,4.

Greaves's meaſure of the door-caſe of the Pantheon, 9 inches within the jambs, is 19,602 of his London feet and decimal parts (7). Deſgodetz found the meaſure of the ſame on the inſide 18 feet $4\frac{7}{8}$ inches Paris meaſure, and on the outſide 18 feet $4\frac{1}{3}$ inches, their difference is $\frac{1}{2}\frac{3}{4}$ of a Paris inch; the depth of the jambs is 4 feet $10\frac{3}{4}$ inches Paris meaſure; and

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(6) Eſattiſſimo piede Ingleſe. See Saggi di Diſſertazioni Academiche di Cortona, vol. iii. p. 113. I have been credibly informed, that this London foot was given him by the late Martin Folkes, Eſq; and that it was made by Siſſon, according to Graham's meaſure.

(7) Greaves, p. 348.

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9 London inches are equal to 8,447 Paris inches and decimals. Hence the meaſure of this door-caſe 9 London inches within the jambs, ſhould, according to Deſgodetz, be 18,3676 Paris feet and decimals. But 19,602 is to this number, as 1065,4 to 998,3; and Greaves's London foot wanted $1\frac{7}{10}$ to be to Deſgodetz's Paris foot, in the proportion of 1000 to 1065,4.

Greaves ſays, moſt of the white marble ſtones on the pavement of the Pantheon contained exactly 3 of thoſe Roman feet on Coſſutius's monument (8). Their Paris meaſure, according to Deſgodetz, is 2 feet $8\frac{1}{2}$ inches, which is equal to 2885,46 London parts. This number divided by 3, gives but 961,82 ſuch parts; whereas Greaves's meaſure of the Coſſutian foot was 967 parts of his London foot; which muſt therefore have wanted above 5 parts in 1000 of the proportion before-mentioned. But it is probable they meaſured different ranges of theſe ſtones.

The meaſure of the Paris foot, which Greaves received from Monſ. Hardy, was taken from the old ſtandard in the Chatelet (9), and contained 1068 ſuch parts as his London foot contained 1000. Theſe numbers are in the proportion of 1065,4 to 997,57; therefore, if the new ſtandard did not differ from this old one (and no ſuch difference appears to have been intended), Greaves's London foot muſt have been 2,43 parts in 1000 ſhorter than Graham's.

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(8) Greaves, p. 211.

(9) See Monſ. Hardy's Letter, in Greaves's Works, p. 447. The meaſure he ſent was only half the Paris foot; but he tells Greaves, it was taken from the ſtandard, under his own inſpection, with great care.

All

All theſe compariſons ſhew Greaves's meaſure of the London foot to have been ſhorter than Graham's. But they are not all of equal authority. The meaſures of Snellius depend upon the correctneſs of three ſeveral feet; that is to ſay, his copy of the iron ſtandard, the Rhynland, and the Paris foot; whereas the reſt (except Norwood's) depend only on the Paris foot, applied to the ſame magnitudes that Greaves himſelf had meaſured. Norwood's meaſure is rather to be wondered at, that, notwithſtanding all the difficulties he had to encounter, it ſhould come ſo near to the true magnitude of a degree, than to be depended on for the exact meaſure of the iron ſtandard; and is an inſtance of what the diligence and ſagacity of a private and obſcure man, unaſſiſted by the public purſe, or the contributions of friends, could accompliſh; whoſe labours may, perhaps, be forgotten, when the meaſures of the French, taken under a royal commiſſion, ſhall be known to the lateſt poſterity.

The three more immediate compariſons of Greaves's meaſure with the Paris foot, are by the meaſures of Auzout, Deſgodetz, and Hardy, which afford as clear a proof as can well be expected in this matter, that his meaſure of the iron ſtandard was about 2 parts in 1000 deficient of Graham's London foot.

Mr. Greaves found the foot on the monument of Statilius to contain 972 ſuch parts as his London foot contained 1000; and that on the monument of Coſſutius 967 (1). Theſe meaſures reduced to Graham's foot, are 970, and 965.

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(1) Greaves, p. 208, 209.

Auzout's

Auzout's meaſure of the foot on the monument of M. Ebutius, is nearly equal to 973 London parts. Revillas makes this 972¼, and the Capponian foot 968¾.

The mean of theſe meaſures of the Roman foot on the marbles, is nearly 969 thouſandth parts of the London foot.

Philander, in his notes on Vitruvius (2), ſays, the foot from a porphyry column, inſcribed ΠΟΔ. Θ. exceeded the Coſſutian foot by the ninth part of an inch; therefore it contained nearly 974 London parts. This column ſeems to have been a more authentic meaſure of the Roman foot, than the figures on the monuments; ſince the inſcription ſhews it was intended for ſuch, whereas they were only repreſentations of it. And notwithſtanding the inſcription was Greek, the foot muſt have been Roman; for Philander's meaſure of it exceeds the mean meaſure of thoſe on the marbles, by leſs than the ſixteenth part of a London inch, and is leſs than ſome of the antient foot-rules, hereafter mentioned; whereas it wants above the third part of an inch to be equal to the Greek foot. It might, perhaps, be uſed by ſome Greek maſon at Rome, to adjuſt his workmens rules by; for when the Romans began to be expenſive in their buildings, they had moſt of their maſons from Greece. And though this column is not now to be found, and the accuracy of Philander's meaſure of it may be doubted, it is ſtill a proof, that, at one time or other, the meaſure of the Roman foot was ſenſibly greater than that on the monument of Coſſutius.

(2) P. 117. of the Lyons edit. 1552.

Lucas

Lucas Pætus compared five antient foot-rules with the marbles; he ſeems not to have been a perſon of great accuracy; for though in one place he allows there was ſome ſmall difference between the Statilian foot and that on the monument of Coſſutius (3), yet he every-where elſe ſpeaks of them as equal, and has given but one figure for the meaſure of both. His compariſons ſeem to have been made only with the latter, the meaſure of which he had before ſet up in the Capitol, as the true Roman foot (4). Three of theſe rules, which, he ſays, were exactly equal to each other, were ſhorter than the Coſſutian foot by one ſeventh part of a Coſſutian inch; they muſt then have been nearly 953$\frac{1}{7}$ London parts. Of the other two, one appears to have been 958, and the other 974, equal to that from Philander's column. He decides in favour of the three ſhorteſt, to which perhaps he was induced, partly by their agreement with each other, and partly by his opinion, that the Roman foot had been continually increaſing; whence it muſt follow, that the ſhorteſt meaſure of it was the moſt antient.

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(3) Cum quo ſane convenit modico diſcrimine hic Statilianus. Pætus de Menſ. et Pond. lib. i.

(4) Greaves, p. 209. ſays, the Roman foot engraved by Pætus in the Capitol, agreed exactly with that on the monument of Coſſutius. Pætus therefore ſeems at that time to have acquieſced in the opinion of Portius, which, he ſays, was generally received. (Leonardus Portius Vicentinus, vir ſane doctus, primus omnium noſtræ ætatis qui de hac re ſcripſerit, menſuram Colotiani pedis pro vera tradidit.—Quem juniores quoque ſecuti ſunt.) But he changed his opinion when he wrote his book, which was after he had ſet up the meaſures in the Capitol.

Greaves ſays, the Coſſutian foot exactly agreed with ſome very antient and perfect Roman feet in braſs; and from his compariſon of two that differed from it (which ſeem to be the greateſt and leaſt he had met with), one of them appears to contain nearly 972¾ London parts, and the other 962⅗ (5).

P. Revillas has given the meaſures of three foot-rules of an uncommon length; one of which contains 972 London parts, another 976, and the third 983 (6).

There are many more of theſe rules in being, that we have no account of; for moſt of the writers on this ſubject, preferring the authority of the marbles, have been negligent in their accounts of the foot-rules; therefore I think a mean meaſure from theſe above-mentioned of no great authority, eſpecially as ſome of them have been produced only on account of their difference from the more common meaſure of the reſt.

If a cubic Roman foot contained a quadrantal of wine, the cube of half a foot muſt have contained a congius, which was the eighth part of the quadrantal. Pætus, Villalpandus, and Auzout meaſured the ſtandard congius of Veſpaſian with water; and have

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(5) Greaves, p. 222. But, p. 227. he ſays, there is not one of ten meaſures of the Roman foot (beſides thoſe on the monuments), that arrive to the proportion of that deduced by Villalpandus from the congius, by 27 parts in 2000; whereas the greater of the two here mentioned contained 974¾ parts of his London foot, and he makes the foot from the congius of Villalpandus, but 986 ſuch parts; their difference is but 23 two thouſandth parts of the latter.

(6) Saggi di Diſſ. Academ. di Cortona, vol. iii.

given

given the weight of its contents. The two firſt differ from each other by almoſt 5½ modern Roman ounces, which is nearly the one-and-twentieth part of the whole weight, though they both meaſured up to the ſame part of the neck. The weight of water given by Pætus, is deficient of the mean weight of Auzout's meaſures by 3 Troy ounces; and that given by Villalpandus exceeds the ſame by 2; therefore they muſt both have uſed bad ballances.

Auzout meaſured this veſſel twice, with the water of Trevi. His greater weight of the water is 63024 Paris grains, his leſſer 62760 (7).

Picard found a cubic Paris foot of ſpring water to weigh 641326 Paris grains (8); and that foot containing 2089,69 cubic London inches, Auzout's greater weight will give 205,36 ſuch inches for the ſolid content of the congius, his leſſer 204½.

According to Picard's experiment, a cubic London inch of ſpring water ſhould weigh but 251¾ Troy grains; whereas, according to Snellius, it ſhould weigh above 254 (9).

Our countryman Wybard made many experiments to diſcover the weight of water (1), from which the weight of a cubic inch of rain water ſhould be 253 Troy grains. His experiments ſeem to have been made with great attention, and his inch muſt have been that of the iron ſtandard, which I have already

(7) See the paper De Menſuris, above quoted, p. 366. 371. of the folio edit. where Picard gives theſe weights as Auzout's, without any farther account of them.

(8) Ibidem.

(9) Snellius, in Eratoſthene Batavo, p. 155.

(1) See Wybard's Tactometria, from p. 269 to p. 287.

ſhewn to be leſs than Graham's; therefore, I think a cubic inch of Graham's meaſure of the water of Trevi, cannot weigh leſs than 253 Troy grains. This will reduce Auzout's two meaſures of the congius to 204,35 and 203,49 cubic London inches; which give 981,7 and 980,3 for the meaſure of the Roman foot.

Greaves meaſured the congius with millet ſeed (2), and comparing it with our meaſures of capacity, found it to contain $7\frac{1}{8}$ pints wine meaſure, and $6\frac{1}{6}$ pints corn meaſure. When he wrote, the wine gallon was univerſally allowed to contain 231 cubic inches (3); and the dimenſions of the buſhel were publiſhed yearly, by the Lord Mayor of London, to be 19 inches in diameter, and $7\frac{1}{2}$ in depth (4). But Greaves's inches were according to the iron ſtandard in the Guildhall, and were 2 parts in 1000 ſhorter than Graham's. Therefore reducing theſe meaſures in that proportion, the wine gallon will be found to contain 229,62 cubic London inches, and the corn gallon 264,22. Hence by the wine meaſure, the congius contained 204,5 cubic London inches, and by the corn meaſure 203,7; which give 981,9 and 980,6 for the meaſure of the Roman foot.

Theſe meaſures agree ſo well with thoſe of Auzout, that I think a mean between them muſt be very near the truth, and that we may reckon the

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(2) Greaves, note *f.* p. 303.

(3) See Wybard's Tactometria, p. 264. Oughtred's Circles of Proportion, p. 57. who quotes Gunter and Briggs for the ſame opinion.

(4) Wybard's Tactometria, p. 282.

foot,

foot from the congius to contain about 981 London parts.

Greaves took likewiſe another meaſure of this congius, by the cube of his Coſſutian foot, which he found to contain 7 congii, and about an half, of millet (5). Now, the quadrantal ſhould contain 8; hence the ſide of the quadrantal to this congius will contain 988 parts of the iron ſtandard: for 988 is to 967 (Greaves's meaſure of the Coſſutian foot) in the ſubtriplicate proportion of 8 to $7\frac{1}{2}$. But this is a greater meaſure than even that of Villalpandus, and was probably owing to an erroneous cube of the Coſſutian foot; for the joiners at Rome might be as bad workmen as the ſcalemakers; and Greaves ſays, he could not get a ballance there, fit to examine the congius by weight (6).

Monſ. Aſtruc, in his Natural Hiſtory of Languedoc, ſays, he had cauſed the diſtance between the ninth and tenth mile-ſtones on the road from Niſmes to Beaucaire to be meaſured, and that it was found to be 754 Paris toiſes. They were both ſet up by Tiberius, and the road lies in a ſtrait line between them. This meaſure gives a Roman foot of 964 London parts. He does not ſay he took this meaſure himſelf (7), nor has he given any account how it was taken. Perhaps by a provincial meaſure, and

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(5) Greaves, p. 225.

(6) Ibid. note f.

(7) Memoires pour l'Hiſtoire Naturelle de Languedoc, p. 225. His expreſſion is, " J'ai fait meſurer la diſtance de l'un à l'autre, et elle s'eſt trouvée de 754 toiſes de Paris." Where he ſpeaks of the Marquis Maffæi, he ſays, " Il à meſuré;" but, in the ſame paragraph, ſpeaking of himſelf, he again ſays, " J'ai fait meſurer."

after-

afterwards reduced to Paris toiſes; or by a wheel, which is liable to great errors: therefore we cannot allow much authority to it, unleſs we were better aſſured of its accuracy. He quotes the Marquis Maffei for a meaſure of 756 toiſes between two other mile-ſtones on the ſame road; but obſerves, that they were ſet up by different emperors, Auguſtus and Tiberius, and that the road formerly made an elbow between them, whoſe poſition is now not certainly known. Therefore this meaſure, which gives a foot of 966½ London parts, is likewiſe of ſmall authority.

All that can be determined from ſuch uncertain and diſcordant data, as I have here collected, is a meaſure that ſhall probably be neither the greateſt nor the leaſt magnitude of the Roman foot. And for this, I ſhall take a mean from all the meaſures above recited, which is nearly 968 thouſandth parts of the London foot.

Before I enter upon the examination of the antient buildings, it may be proper to ſay ſomething concerning the nature of the evidence to be expected from them.

All buildings are planned and executed by ſome meaſure of the country where they are built. At Rome this meaſure was the foot, which was divided by the workmen into 4 palms, and each palm into 4 digits (8).

If

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(8) Vitruvius, lib. iii. c. 1. Frontinus de Agrorum Qualit. Both theſe authors are technical writers, and give this as the diviſion uſed by workmen; and the antient foot-rules are ſo divided.

They

If the Roman buildings were correctly executed, and we had the true dimenſions of their ſeveral parts in any known meaſure, ſome diviſors conſiſting of Roman feet, and parts of thoſe feet, applied to theſe meaſures, muſt, in the ſame building, give the ſame quotient to all; and this quotient will be the meaſure of the foot, by which that building was conſtructed, in parts of the known meaſure. Therefore, where a range of ſimple diviſors, applied to the principal parts of any building, give as nearly the ſame quotient as can be expected from the common inaccuracies of workmanſhip, we may reaſonably conclude, that theſe diviſors were the architects numbers; and the foot derived from them, that by which the building was conſtructed.

As an architect cannot be ſuppoſed to be limited to a few digits in the extent of the front, or of the depth of large buildings, it is probable ſuch meaſures conſiſted of whole feet. Theſe, and the diameters of circular buildings, I call prime meaſures.

In all large prime meaſures, the preference is to be given to a round number for the diviſor; as it is more probable a building ſhould be deſigned for 100 feet in front, than for 99 or 101: and becauſe the paſſus was 5 feet, I reckon any multiple of 5 a round number.

The diameters of columns are of leſs authority than any other horizontal meaſures; not only on account of the difficulty of meaſuring them correctly,

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They both mention the duodecimal diviſion, which ſeems to have been uſed by the vulgar; for the Romans divided every integer into 12 unciæ.

but

but becauſe errors of workmanſhip, to which they are more liable than ſquare members, more ſenſibly affect the magnitude of the foot in ſmall meaſures, than in large ones.

Uprights, of any conſiderable height, are of leſs authority than horizontal meaſures, from the difficulty of taking them correctly; and being deſigned by modules, few of them anſwer well to the foot meaſure. But here we muſt except ſuch ſhafts of columns as are of one block of marble; which ſeem to be as good authority as any part of a building: for the neceſſity of making them all exactly of the ſame length, muſt produce accuracy; and the doing this was no difficult piece of workmanſhip. Being likewiſe commonly (if not always) wrought at the quarry, to ſave expence in the carriage, they were probably beſpoke to ſome ſimple meaſure; and we ſhall find all ſuch ſhafts anſwer to ſome number of whole palms.

In the following enquiry, I ſuppoſe that Deſgodetz's Paris foot contained 1065,4 ſuch parts, as our London foot contains 1000: ſince it appears, from the compariſon of his meaſure of the door-caſe of the Pantheon with Greaves's, that if Greaves's London foot was not above 2 parts in 1000 ſhorter than Graham's, Deſgodetz's Paris foot was but $\frac{3}{10000}$ of the London foot ſhorter than that in the archives of the Royal Scociety. This difference (if ſuch there was) does not amount to the 277th part of an inch; which is much nearer than we can expect to find the meaſure of the Roman foot, from all the remains of antiquity now in being.

The

The temple of Fortuna virilis.

When this temple was built, is not known; but it is commonly thought, from the poorneſs of the materials, and the ſtyle of the architecture, to be one of the moſt antient buildings now remaining in Rome. The great ſimplicity of its parts, and their near agreement with each other, afford ſo clear an evidence of the meaſure by which it was conſtructed, that I have choſen to begin with it, as an introduction to the reſt.

It has four Ionic columns in front, and ſeven in depth.

The depth, taken between the centers of the angular columns, is 54 feet $8\frac{1}{4}$ inches Paris meaſure, equal to 58264,06 thouſandth parts of the London foot; which contain juſt 60 Roman feet of 971 + ſuch parts each.

The diſtance between the centers of the columns in this range, is 9 feet $1\frac{3}{8}$ inches, equal to 9710,6 London parts; containing 10 Roman feet of the ſame meaſure with the former.

The extent of the front, taken between the centers of the angular columns, is 28 feet $8\frac{1}{6}$ inches, = 30556,25; which contain $31\frac{1}{2}$ Roman feet of 970 + London parts each.

The diſtance between the centers of the angular columns, and thoſe on each ſide the entrance, is exactly equal to the diſtance between the centers of the columns on the ſides; being 10 Roman feet of 971 + parts each.

There remain therefore $11\frac{1}{2}$ Roman feet, for the diſtance between the centers of the columns on each

ſide the entrance. This meaſures 10 feet $5\frac{5}{12}$ inches (9), equal to 11134,9; which, divided by $11\frac{1}{2}$, gives $968\frac{1}{4}$ for the Roman foot.

This diſagreement between the meaſures in the front, cannot be reconciled by any probable diviſors; therefore it muſt be owing to an error in workmanſhip. And if the diſtance between the centers of the middle columns were but a little more than $\frac{3}{8}$ of a London inch greater, both that, and the whole extent of the front, would anſwer to the ſame meaſure of the foot with the ſides. Now, as the middle intercolumniation was intended to be greater than the reſt, the workman might be leſs exact in laying out the front, than the ſides, where they were all to be equal; for the front intercolumniations next the angles being kept equal to thoſe on the ſides, the ſymmetry of the building would be preſerved, and the whole error fall on the middle intercolumniation, where it could not be diſcovered.

Or if, on the other hand, we ſuppoſe the meaſure of the extent in front to have been correct, and the error to have lain in the ſides, the workman muſt ſtill have made the intercolumniations on the ſides equal to each other, and thoſe in the front next the angles, equal to them: and, in this caſe, 970 would be the true meaſure of the foot, inſtead of 971.

Therefore, I think, it cannot be doubted, that one or the other of them was the meaſure by which this building was conſtructed.

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(9) Deſgodetz has figured this 10 feet $5\frac{1}{12}$ inches; which will be found to be a miſtake, by comparing the correſpondent numbers, and by his meaſure of this interval in modules.

The

The temple of Veſta, at Rome.

This building is remarkable for the ſtyle of its architecture, and the elegance of its workmanſhip. The pointed abacus ſhews the architecture to be Greek; which is a ſtrong preſumption in favour of its antiquity, as it is the only example of the kind now remaining at Rome (1).

None of the diameters will anſwer to the ſame meaſure of the foot with the other parts, by probable diviſors. This was likely enough to be the caſe, if the circles of the plan were deſcribed by a cord, whoſe ſtretching might increaſe their diameters beyond the intended meaſures. But the reſt of the meaſures agree pretty nearly with each other, by as ſimple diviſors as can be expected in ſo ſmall a building.

At the door-caſe, the arch of the wall is diſcontinued, and the groundſil is ſtrait. The width between the jambs is 9 feet $1\frac{2}{3}$ inches, equal to 9736,6; which, divided by 10, gives a foot of 973,7 London parts.

From the wall to the centers of the columns, is 8 feet $8\frac{1}{8}$ inches, = 9244,6; which, divided by $9\frac{1}{3}$, gives a foot of 973,1 parts.

From the wall to the extremity of the baſement, is 10 feet 6 inches, = 11186,7; which, divided by $11\frac{1}{2}$, gives 972,7.

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(1) That is to ſay, it is probably older than the Pantheon; for, according to Pliny, in his thirty-ſixth book, there ſeems to have been no marble temple in Rome above ſixty years older than that building. And his hiſtory of marble is confirmed by the ſaying of Auguſtus, "That he found Rome brick, and left it marble." Sueton. in Aug. c. 28.

From the top of the ſurbaſe within, to the pavement of the cell, is 7 feet $9\frac{1}{6}$ inches, = 8271,65; which, divided by $8\frac{1}{2}$, gives 973,1.

From the top of the ſurbaſe without, to the outer pavement, is 9 feet $1\frac{5}{6}$ inches, = 9751,4; which, divided by 10, gives 975,1.

The ſhafts of the columns are 27 feet 5 inches, = 29209,8; which, divided by 30, gives 973,7.

The columns, with their baſes and capitals, are 32 feet 0 inch, = 34092,8; which, divided by 35, gives a foot of 974 London parts.

The mean meaſure of the foot from this building, is 973,6; which agrees nearly with the foot of Philander's porphyry column above-mentioned, and with one derived from Greaves's meaſure of the monument of Ceſtius, whoſe ſide within the city (as he ſays in p. 151 of his works) is completely 78 feet Engliſh; which, reduced to Graham's meaſure, is 77,84, and contains 80 Roman feet of 973 London parts each.

The temple of Veſta, at Tivoli.

The meaſures of the parts of this building diſagree ſo much with each other, by any probable diviſors, that I ſhall only mention the width of the door-way between the jambs, which is 7 feet $3\frac{3}{4}$ inches, = 7790,7, and anſwers to 8 Roman feet of 973,8 parts each.

The Pantheon.

This is a circular building, with a portico before the entrance; which, having been added after the body of the work was finiſhed (2), has induced many

(2) See Deſgodetz, p. 3. & p. 14.

to think the temple was more antient, and that Agrippa only adorned it with marble, and added the portico. But the inſcription on the freze, " M. " Agrippa L. F. Coſ. tertium fecit," ſeems to refer to the whole. And Pliny, enumerating the moſt magnificent buildings in Rome, ſays, " Pantheon " Jovi ultori ab Agrippa factum; cum Theatrum " ante texerit Romæ Valerius Oſtienſis architectus, " ludis Libonis (3)." If the latter part of this paſſage alludes to the dome of the Pantheon (as it ſeems to do), it will imply, that Agrippa built it. In another place (4), Pliny calls this building Agrippa's Pantheum. Now, temples of this form were uſually peripteres; that is to ſay, encompaſſed with columns (5), as the two temples of Veſta; but ſuch a colonnade to this building (ſuppoſing the entablature to reach up to the top of the wall) could not conſiſt of fewer than twenty-ſix columns of 9 London feet in diameter; which, with their entablature, muſt have coſt much more than the whole building, as it now ſtands. Therefore, it ſeems as if no columns were at firſt intended on the outſide, and that the portico was an after-thought.

This temple is ſaid to have been burnt in that great conflagration, which happened in the reign of Titus (6); and again by lightning, in the thirteenth year of Trajan (7), and to have been reſtored by

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(3) Pliny, lib. xxxvi. c. 15.
(4) Ibid. c. 5.
(5) Vitruvius, lib. iv. c. 7.
(6) Xiphilin. in Tito. Suetonius likewiſe mentions this fire; but does not ſay what buildings were burnt by it. Titus, c. 8.
(7) Euſebius in Chronico.

Adrian

Adrian (8). But it does not ſeem to have been totally deſtroyed either time; for the inſcription of Severus ſays, it was " vetuſtate corruptum;" which could hardly be true, if it was entirely rebuilt by Adrian, who began his reign but ſeventy-ſix years before Severus; but is very applicable to a building that had ſtood above 200 years, and been twice damaged by fire. Neither does it ſeem poſſible, that a building of this ſize and ſolidity, without any timber in it, except the roofing of the portico, ſhould be totally conſumed by the fire of the neighbouring buildings. Therefore, we may ſuppoſe the meaſures of the principal parts of it to be according to the foot of the Auguſtan age.

If the execution of this noble building had been anſwerable to the beauty of the deſign; the multiplicity of its parts, and the accuracy with which Deſgodetz has meaſured them, would have afforded ample materials for the diſcovery of the foot by which it was conſtructed: but the dimenſions of ſuch parts of it as ſhould be equal to each other, differ ſo widely, that ſcarce any but the prime meaſures are to be depended on.

Theſe were, probably, the diameter of the circle paſſing through the centers of the columns within; for on this, the diameter of the cupola depended; and the diameter of the whole building, from out to out.

Deſgodetz has given twelve meaſures of the diameter within the ſhafts of the columns. The mean from them all, is 133 feet $2\frac{2}{5}$ inches Paris meaſure:

(8) Spartianus in Adriano.

to this, add the mean diameter of the columns, 3 feet $4\frac{4}{5}$ inches, their ſum, 136 feet $7\frac{1}{5}$ inches, equal to 145533,6 London parts, is the diameter of the circle paſſing through the centers of the columns; which, divided by 150 (as round a number as can be deſired), gives 970,2 ſuch parts for the meaſure of the Roman foot.

The mean depth of the four long chapels, between the ſhafts of the columns and the wall, is 9 feet $1\frac{47}{48}$ inch, = 9764,3; which, divided by 10, gives a foot of 976,4 London parts; but by $10\frac{1}{4}$, one of 952,6; which is certainly too ſmall. And if this was an arbitrary meaſure, we cannot admit the fraction of a palm in it. Therefore, we may ſuppoſe theſe chapels were intended to be 10 Roman feet deep in the clear between the columns and the wall.

The mean diameter of the columns, 3 feet $4\frac{4}{5}$ inches, = 3622,36 London parts, divided by $3\frac{3}{4}$, gives a foot of 966 parts. The ſhafts of theſe columns meaſure 27 feet $1\frac{1}{3}$ inches, whoſe eighth part (reckoning the ſhaft at 16 modules) is 3 feet $4\frac{2}{3}$ inches; which is ſomewhat leſs than the mean of their diameters. Yet even this meaſure (which is equal to 3610,5 London parts) does not want half a Roman digit of $3\frac{3}{4}$ feet of 970,2 parts each; which the architect might ſafely neglect, and allow $3\frac{3}{4}$ feet for theſe diameters, in his general plan.

Now, if to twice the depth of theſe chapels, or 20 Roman feet, we add twice the ſemidiameter of the columns, or $3\frac{3}{4}$ ſuch feet; and to their ſum, the diameter of the circle paſſing through the centers of the columns, 150 feet; we ſhall have $173\frac{3}{4}$ Roman feet

feet for the diameter from wall to wall of the oppoſite chapels.

Deſgodetz has given four meaſures of this diameter, whoſe mean, 158 feet $2\frac{2}{3}$ inches, is equal to 168570 London parts, which, divided by $173\frac{3}{4}$, give 970,2 —, for the meaſure of the Roman foot; agreeing with that found from the diameter of the circle paſſing through the centers of the columns.

Or if, inſtead of taking a mean of the four diameters from wall to wall, we add twice the mean depth of theſe chapels, and twice the mean ſemidiameter of the columns, to the mean diameter of the circle paſſing through their centers, we ſhall have 158 feet 4 inches for the diameter from wall to wall, equal to 168688 London parts; which, divided by $173\frac{3}{4}$, give 970,9, for the meaſure of the foot.

But if this diameter from wall to wall was a prime meaſure, as well as that of the circle paſſing through the centers of the columns, the depth of theſe chapels muſt have been a reſiduary meaſure, and might be more or leſs than 10 Roman feet. And if we divide the diameter laſt mentioned by 174 (which being compoſed of 150, and twice 12, may here be reckoned a round number), it will give a foot of $969\frac{1}{2}$ London parts. But if, inſtead of twice the mean depth of theſe chapels, we take twice the greateſt depth, 18 feet 6 inches, for part of this diameter, the ſame diviſor will give a foot of $970\frac{1}{2}$ ſuch parts.

The meaſure of the diameter from out to out cannot be ſo well determined as either of the former; for Deſgodetz has given but one meaſure of the outer wall, which is 5 feet $4\frac{1}{2}$ inches; to this we muſt add 1 foot $10\frac{1}{3}$ inches, for the mean depth of the niches

in

in the long chapels; and twice their ſum, 14 feet $5\frac{2}{3}$ inches, added to 158 feet $2\frac{2}{3}$ inches, the inſide diameter between the walls of theſe chapels, will give 172 feet $8\frac{1}{3}$ inches for the diameter from out to out. This is equal to 183988,6 London parts; which anſwer to 190 Roman feet, of 968,4 ſuch parts each. But this diameter being got by the addition of many ſmall parts, one of which is a ſingle meaſure, is of ſmall authority. Though if the outer wall is 5 feet $4\frac{1}{2}$ inches thick in brick-work, and the backs of the niches in the long chapels are incruſted with marble (as I ſuppoſe they are), a proper allowance for the incruſtation will make this diameter anſwer to a foot of 970 London parts.

The principal parts of the circular range within, are too unequal to anſwer to any regular diviſion into compartments; and the two largeſt meaſurēs in it ſufficiently evince, how little uſe is to be made of the reſt: for the width of the grand chapel oppoſite the entrance, and the width of the entrance between the pilaſters in the range, will not anſwer to the ſame meaſure of the foot, by any probable diviſors.

But the inequality of the faces of the pilaſters, that terminate the ſolids in this range, affords the cleareſt proof, that no certain meaſure of the foot is to be obtained from the leſſer parts of this building: for two of them, at the extremities of the ſame ſolid, differ in breadth by a Paris inch and three quarters. One of the two on each ſide the grand chapel exceeds the other in breadth, by a Paris inch and a quarter; and it exceeds the leaſt in the circular range, by no leſs than $4\frac{1}{4}$ ſuch inches, though the diameter of the column before it is leſs than that of ſome in the range.

The diameter of the circular light at top is 27 feet 5 inches, equal to 29209,7; which, divided by 30, gives 973⅔ for the Roman foot. Whether Deſgodetz took more meaſures than one of this diameter, does not appear, nor how he took it. It is probable, that 30 Roman feet was the meaſure here intended, being a round number, and one fifth of the diameter between the centers of the columns: but the making this light a true circle, preciſely anſwering to a given meaſure, was a difficult piece of workmanſhip. Therefore, this meaſure is of ſmall authority.

The ſquare compartments on the pavement meaſure 9 feet 0⅔ inch, or 9647,79 London parts; which, divided by 10, give a foot of 964,8 parts. But Deſgodetz obſerves (9), that this is not the original pavement: and this meaſure of the foot anſwers to the age of S. Severus, as I ſhall ſhew hereafter.

This pavement being conſiderably raiſed above the old one, no meaſures of uprights taken from it are of any authority.

The height of the attic, above the cornice it ſtands upon, is 27 feet 2¾ inches, equal to 29010; which, divided by 30, gives a foot of 967 parts. But the walls of this building being brick, this meaſure may have been diminiſhed by their ſettling, as the whole error occaſioned by it might be made to fall on this part, rather than on the order below.

The length of the ſhafts of the columns in the circular range, is 27 feet 1⅓ inches, = 28884,18; which anſwers to 29¾ feet, of 970,9 parts each.

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(9) Deſgodetz, p. 41.

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The width of the door-caſe, between the jambs, has been ſuppoſed, by Picard, De la Hire, Eiſenſchmid, and others, to have been a meaſure of whole feet; for this reaſon, becauſe they thought it an arbitrary one, and therefore not likely to contain a fraction (1). But in this, I think, they were miſtaken; for it appears to have been determined by the width of the paſſage within, of which it takes up juſt three fourths. The proportion is too ſimple, and too exactly executed, to be accidental; and it is highly improbable, that the width of this paſſage, which is a principal part of the plan of the inſide, ſhould be determined by that of the door-caſe. The width of the paſſage, is 24 feet $5\frac{2}{2}\frac{3}{4}$ inches, = 26098,6; that of the door-way, next the paſſage, is 18 feet $4\frac{7}{8}$ inches, = 19610; next the portico, 18 feet $4\frac{1}{3}$ inches, = 19561,9; none of which will give a meaſure of the foot, anſwering to the reſt of the building, by whole diviſors. The width of the door-way next the portico (which has been ſuppoſed to be juſt 20 Roman feet), divided by 20, gives 978. But the diameters of the temple (taking either the greateſt or the leaſt, inſtead of a mean), will not anſwer to a foot of this meaſure, by probable diviſors.

The principal meaſures in the portico are, its extent in front and depth, and the ſhafts of the columns.

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(1) Mr. Greaves ſays, this door-caſe is one entire marble ſtone. See p. 348. and p. 494. And Ficoroni (a late writer), in his Veſtigia di Roma antica, p. 132. ſpeaking of this door-caſe, ſays, "Eſſendo amendue i lati, col di ſopra, e il di ſotto di un ſolo pezzo di marmo." But Deſgodetz, p. 18. ſays, that each ſide of the architrave conſiſts of two ſtones, and the top, of one; and corrects Serlio, for ſaying the whole was of one piece.

The extent in front, taken between the centers of the angular columns, is 98 feet $4\frac{3}{8}$ inches, equal to 104797,6; which contain 108 Roman feet of 970,3 parts each. Its depth on the right-hand ſide of the entrance (2), from the center of the angular column to the center of the pilaſter, is 41 feet $5\frac{1}{8}$ inches, = 44136,4, and contains $45\frac{1}{2}$ feet of 970 + parts. The ſhafts of the columns, which are each of one ſingle ſtone, meaſure 36 feet $7\frac{7}{8}$ inches (3), equal to 39053,6; which anſwer to $40\frac{1}{4}$ Roman feet of 970,3 London parts.

I can find no meaſures in this building of equal authority with the diameters, the ſhafts of the columns in the circular range, and theſe meaſures in the portico. All of which give the meaſure of the Roman foot greater than 970 ſuch parts as the London foot contains 1000.

In the reſt of the buildings, where the meaſures do not require a particular explanation, I ſhall firſt give Deſgodetz's meaſures, with their reductions to thouſandth parts of the London foot, and then ſet down the diviſors and quotients with two hooks between them; the diviſor on the left-hand, expreſſing

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(2) By ſome expreſſions of Deſgodetz, his plans ſeem to be inverted, as gravings commonly are. I ſpeak according to the plan, ſuppoſing the reader to view it in front.

(3) Deſgodetz, p. 19. has figured the ſhafts of the pilaſters in the portico 36 feet $7\frac{1}{3}$ inches, and in modules 16 mod. $8\frac{1}{4}$ parts; which is ſomewhat too great for his foot meaſure, his module being 27 Paris inches. But p. 16. he has figured the ſhafts of theſe columns 16 mod. $8\frac{3}{4}$ parts, which is 36 feet $7\frac{7}{8}$ inches. And that this is no miſtake, appears from his meaſures of the capitals of each.

a num-

a number of Roman feet; and the quotient on the right, their magnitude in parts of the London foot.

The temple of Peace.

The length on one ſide within the walls, including the pronaus or porch, is 269 feet $8\frac{1}{2}$ inches, = 287347,26 ——— ———	296) ... (970,8 —
The width of the nave 77 feet $5\frac{1}{2}$ inches, = 82524,1 (twice) ———	85) ... (970,9 —
The width of the ſide-building 48 feet 0 inch, = 51139,2 (thrice) ———	$52\frac{3}{4}$) ... (969,5 —
The length of the firſt diviſion of the ſide 71 feet 5 inches, = 76087,3	$78\frac{1}{2}$) ... (969,3 —
The length of the ſecond diviſion 71 feet 8 inches, = 76353,87	$78\frac{3}{4}$) ... (969,6 —
The length of the third 70 feet $3\frac{1}{2}$ inches, = 74888,75 ———	$77\frac{1}{4}$) ... (969,4
The width of two of the paſſages in the croſs walls, is 22 feet 4 inches, = 23793,9 ——— ———	$24\frac{1}{2}$) ... (971,2 —
The width of the exedra in the middle diviſion of the ſide 53 feet 11 inches, = 57442,82 ———	$59\frac{1}{4}$) ... (969,5
Its depth is equal to the width of the paſſages in the croſs walls.	
The two middle entrances through the porch, 15 feet 3 inches, = 16247,35 ——— ——— ———	$16\frac{3}{4}$) ... (970 —
The ſide entrances 14 feet $4\frac{1}{2}$ inches, = 15315,13 ——— ———	$15\frac{3}{4}$) ... (972,4 —

The entrance into the porch on the right-hand 14 feet 4 inches, = 15270,74	——	$15\frac{3}{4}$) . . . (969,5
The ſhaft of the column (of one ſtone) 49 feet 3 inches, = 52470,9	——	54) . . . (971,7 —
The diameter of the column 5 feet $8\frac{1}{4}$ inches, = 6058,47	——	$6\frac{1}{4}$) . . . (969,4 —
The height of the order 71 feet $0\frac{1}{2}$ inch, = 75687,79	——	78) . . . (970,4

A mean from theſe twenty-one meaſures gives a Roman foot of 970,3 parts. But the meaſures in this building ſeem to be very incorrectly executed.

The temples of Bacchus, and of Faunus.

I ſhall not particularly recite the meaſures of theſe buildings, becauſe they ſeem to be the works of a much later age than any of the reſt. By the different workmanſhip of the ſhafts of the columns in the temple of Bacchus (4), it ſeems to have been compiled out of the ruins of other buildings. In the temple of Faunus, moſt of the columns are of granate, but ſome of them of white marble; the croſs wall was evidently deſigned to ſupport a flat roof, the outer wall being too weak to bear a cupola (5); and the windows (which I take to be as old as the reſt of the building) are in the Gothic ſtyle: all which are marks of a late age. Therefore both theſe temples ſeem to be more modern than they are commonly thought to be. The temple of Bacchus

(4) See Deſgodetz, p. 68.
(5) Ibid. p. 79.

anſwers

anſwers to a foot of 968 parts, that of Faunus to one leſs than 967.

The amphitheatre of Veſpaſian.

The plan of this building is elliptical; and the area in the middle being allotted to the exhibition of the ſhews, ſeems to be the part on which the magnitude of the whole depended. Therefore one of its axes muſt be the prime meaſure.

The greater axis meaſures 263 feet 11 inches, = 281176,8 London parts; the leſſer 165 feet 1 inch, = 175879,7. Theſe numbers are ſo nearly in the proportion of 8 to 5, that there is little reaſon to doubt their being intended for it.

The only two whole numbers in this proportion that will anſwer our purpoſe are 288 and 180, by which the greater axis will give 976,3 London parts for the meaſure of the foot; the leſſer, 977,1.

Either the diſtance from the inner to the outer wall, or that from the inner wall to the extremity of the upper ſtep, was probably a prime meaſure too. The former is 155 feet $5\frac{1}{3}$ inches, = 165610,5; which, divided by 170, gives 974,2. The latter is 158 feet $0\frac{1}{12}$ inch, = 168340,6; which, divided by 172, gives 978,7.

But the axes will admit of other diviſors, which are in my opinion as probable at leaſt, as thoſe above-mentioned. For if we ſuppoſe the greater axis to have been the prime meaſure from which the leſſer was determined in the proportion of 5 to 8, and aſſume 290 Roman feet for it (which is a more probable meaſure than 288), the leſſer axis muſt then be $181\frac{1}{4}$ ſuch feet. And the meaſure of the foot by the

the greater axis will be 969,6 parts; by the leſſer 970,3.

The diſtance from the inner to the outer wall will not anſwer to this meaſure of the foot by any whole diviſor. And that from the inner wall to the extremity of the upper ſtep, divided by 174, gives but 967½. But this diſagreement will not appear extraordinary, when we conſider that theſe diſtances are not ſingle meaſures, but the ſums of nine or ten ſmaller ones (6), and may therefore be liable to an error of ſome inches.

The leſſer parts are ſo incorrectly executed, that no uſe can be made of them. And the heights of the four orders, and of their parts, are ſo irregular as not eaſily to be accounted for.

The amphitheatre at Verona.

The principal meaſures in this building anſwer to the Greek foot; which is not the ſubject of the preſent inquiry.

The arch of Titus.

This edifice is ſo ruined, that we have not the meaſure of the extent of its front.

The depth of the ſolid is 14 feet 7 inches, = 15537,1 —	——	16)...(971,1 —
The width of the entrance, 16 feet 4 inches, = 17401,5	——	18)...(966,7
The ſhafts of the columns, 16 feet 2 inches, = 17223,97	——	17¾)...(970,9

(6) See the ſection, Pl. 3.

The

The arch of Conſtantine.

The meaſures of this elegant deſign are ſo incorrectly executed, that I can make no uſe of them. The fronts of the two outermoſt ſolids differ by a Paris inch and one third; the ſide entrances by $1\frac{2}{3}$; the diſtances of the columns from the pilaſters (a meaſure of little more than 18 inches) differ an inch and $\frac{1}{12}$; and the depth of the ſolid at one end, exceeds that at the other end by no leſs than $7\frac{1}{4}$ Paris inches.

The temple of Antonine and Fauſtina.

Deſgodetz has given the meaſure of no part of this building in feet and inches, but the diameters of the columns; which he ſays do not differ from each other, as in many of the reſt.

The diameter of the columns is 4 feet $6\frac{7}{12}$ inches, = 4846,09 ———	5)...(969,2
The extent of the portico taken between the centers of the angular columns, 25 mod. $16\frac{5}{6}$ parts, = 61935,7 ——— ——— ———	64)...(967,7
The ſhafts of the columns, 15 mod. 28 parts, = 38607,2 ———	40)...(965,2

Theſe ſhafts were probably intended for 16 modules, and their meaſure is more to be relied on than that of the diameters, both on account of its ſize, and becauſe the correct execution of it was not only more eaſy, but more neceſſary. Therefore, though theſe meaſures are too few, and too unequal to determine the exact magnitude of the foot uſed in the con-

ſtruction of this building, yet it ſeems to have been leſs than any we have hitherto met with.

The arch of Septimius Severus.

This building deſerves our particular attention, both on account of the correctneſs of the workmanſhip, and becauſe it exhibits a different meaſure of the Roman foot from any of the preceding. It is all of white marble. The ſtones are laid without mortar, and faſtened together with cramps. And the ſhafts of the colums are each of one ſtone.

The whole extent in front, is 71 feet $5\frac{5}{6}$ inches, = 76161,3 ———	79)...(964,1
The depth at one end, is 21 feet $8\frac{3}{4}$ inches, = 23150,3 ———	24)...(964,6
The depth at the other end, 21 feet $8\frac{5}{6}$ inches, = 23157,6 ———	24)...(964,9
The width of the middle entrance, 20 feet 10 inches, = 22195,8	23)...(965
The width of the ſide entrances, 9 feet 2 inches, = 9766,17 ———	$10\frac{1}{8}$)...(964,5
From the middle entrance to the extremity of the outer ſolid, on one ſide, is 25 feet $3\frac{3}{4}$ inches, = 26967,9 ——— ———	28)...(963,1
The like on the other ſide, 25 feet $4\frac{1}{12}$ inches, = 26997,5 ———	28)...(964,2
The diſtance between the ſhafts of the outermoſt columns, is 60 feet $7\frac{5}{6}$ inches, = 64619,5 — (7)	67)...(964,5

The

(7) This and the two following meaſures are inſerted only to ſhew the probability, that the diameters of the columns were deſigned

The diſtance between their centers, 63 feet $4\frac{1}{3}$ inches, = 67504,9	70) . . . (964,4
The diſtance including their ſhafts, 66 feet $0\frac{5}{6}$ inch, = 70390,4	73) . . . (964,3
The diameter of the columns, 2 feet $8\frac{1}{2}$ inches, = 2885,46	3) . . . (961,8

The uprights will not anſwer to any one meaſure of the foot by whole diviſors; nor will they agree with each other by broken ones, ſo nearly as the horizontal meaſures do, which may partly be owing to the difficulty of taking them correctly.

The ſhafts of the columns meaſure 21 feet $8\frac{1}{4}$ inches, = 23105,9	24) . . . (962,7
The pedeſtals, 12 feet $3\frac{5}{12}$ inches, = 13088,14	$13\frac{1}{2}$) . . . (969,5
The whole order, 33 feet $4\frac{1}{4}$ inches, = 35535,53	37) . . . (960,4
The attic, 15 feet $5\frac{1}{3}$ inches, = 16454,51	17) . . . (967,9
The blocking, 1 foot $9\frac{2}{3}$ inches, = 1923,64	2) . . . (961,8
The whole height, 62 feet $10\frac{2}{3}$ inches, = 67001,8	$69\frac{1}{2}$) . . . (964,1 —

I have produced theſe uprights, rather as an inſtance how little ſuch meaſures are to be depended on, even in buildings correctly executed, than as of any authority for the meaſure of the foot. For they cannot be reconciled to a common meaſure without

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ſigned to be three Roman feet, and not as of any authority in the determination of the meaſure of the foot.

fractions of the digit in the diviſors, by which means any meaſure of the foot might be derived from any building.

A mean from the ſeven firſt horizontal meaſures gives a Roman foot of 964,3 parts. A mean from the uprights one of 964,4.

But the depth of the ſolid, and the widths of the entrances are moſt to be depended on, and theſe give a foot of $964\frac{3}{4}$ London parts; which will be found to be the greateſt meaſure this building will anſwer to by probable diviſors.

For though the extent of the front, or the width of the middle entrance, muſt probably have been the prime meaſure in a building of this kind; yet no part ſeems more likely to conſiſt of ſome number of whole feet (or at leaſt whole palms) than the depth of the ſolid; ſince there appears no reaſon why the architect ſhould uſe odd digits in the meaſure of it. Now if we ſuppoſe the extent of the front to have been $78\frac{1}{2}$ Roman feet, they will contain 970 parts each; but to make the depth agree with this meaſure, it muſt have been $23\frac{7}{8}$ ſuch feet, which does not ſeem probable, for the reaſon above-mentioned. The middle entrance muſt then have been $22\frac{7}{8}$, inſtead of 23 whole feet, and the extent on each ſide $27\frac{13}{16}$, inſtead of 28. And there will not be one whole meaſure in the plan of this building; which is improbable. Neither do theſe broken meaſures ſerve for the ſubdiviſion of the ſides better, or even ſo well as the whole ones.

The ſhafts of the columns are ſo nearly 16 modules, that they ſeem to have been deſigned for that proportion; and $\frac{1}{32}$ of a Paris inch added to the diameters

meters would make them ſo. But this is not ſufficient to make thoſe diameters anſwer to 3 feet of 964¾ parts, of which they want almoſt $\frac{1}{10}$ of a Paris inch. This may be partly an error in the workmanſhip, and partly in Deſgodetz's meaſure; but if this quantity were added to them, they would not only anſwer to the ſame foot with the reſt of the building, but all the parts in which their meaſure is included, would agree better with the depth of the ſolid, and the widths of the entrances.

The height of the blocking was probably intended for 2 Roman feet, though it anſwers to no greater meaſure of that foot than the diameters of the columns do. But meaſures of 2, 3, and 4 feet, ſeldom agree with the larger ones in any of the buildings; for a ſmall error, either in the workmanſhip, or in the meaſuring, makes a very ſenſible difference in the length of the foot derived from ſuch intervals. And this blocking does not want the $\frac{1}{13}$ part of a London inch, of 2 feet of 965 parts each.

The bankers arch, and the portico of Severus, are contemporary buildings with this, and ſeem to agree with it in the meaſure of the foot. The former is a very ſmall building, and its parts will not anſwer to any one common meaſure by diviſors of whole digits. However, the depth of the ſolid (which, as I have before obſerved, is moſt likely to be a whole meaſure) taken at three different parts, is 6 feet 4 inches, which anſwer to 7 Roman feet of 963,93 London parts.

There are but few meaſures in the portico of Severus. Thoſe moſt to be depended on are, the whole extent in front, and the ſhafts of the columns, which

are

are each of one block. The former meaſures 56 feet $11\frac{78}{100}$ inches, = 60708,3 —, which contain 63 Roman feet of 963,6 parts each. The latter meaſures 26 feet $11\frac{1}{4}$ inches, = 28699,22, which contain $29\frac{3}{4}$ ſuch feet of 964,7 parts. The whole column, with the baſe and capital, meaſures 32 feet $6\frac{1}{2}$ inches, = 34669,9 London parts, which contain 36 Roman feet of 963 parts each.

It appears by the inſcription on the architrave of the portico of the Pantheon (8), that Severus repaired that temple; and Deſgodetz obſerves, that the pavement is not ſo old as the building. That it was laid by Severus is probable, from the correctneſs of its meaſures, which is a remarkable characteriſtic of the workmanſhip of that age. The ſquare compartments on this pavement anſwer to 10 Roman feet, of 964,78 parts, agreeing with a mean from the depth of the ſolid of this arch, and the widths of the entrances, which are the moſt unexceptionable meaſures in it. The breadth of the ſtrait bands between theſe compartments, is equal to the meaſure Deſgodetz has given of the diameters of the columns of this arch, and anſwers to 3 feeet of 961,8 parts. But theſe bands might be purpoſely made ſomewhat ſhort of the full meaſure, to leave room for one of the ſame breadth with the reſt between the outermoſt compartments and the circular range; for notwithſtanding this, the workmen found themſelves obliged to diminiſh both the outermoſt compartment, and the band beyond it.

(8) Imp. Cæſ. Septimius Severus——et Imp. Cæſ. M. Aurelius Antoninus ——Pantheum vetuſtate corruptum cum omni cultu reſtituerunt.

The

The concurring teſtimonies of theſe contemporary works, though not of ſufficient authority to eſtabliſh any meaſure of the foot by themſelves, are yet very ſtrong evidence in favour of the meaſure above derived from this arch. And we ſhall find a farther confirmation of it in Diocletian's baths.

The baths of Diocletian.

The whole length of this building is 439 feet 3 inches, = 467977	485)....(964,8
The length of the great hall, 180 feet $8\frac{1}{6}$ inches, = 192497 +	200)....(962,5
The breadth at one end, 74 feet $3\frac{5}{6}$ inches, = 79179,9	82)....(965,6
Its breadth at the other end, 74 feet $5\frac{1}{2}$ inches, = 79327,9	82)....(967
The diſtance between the middle ſolids on the ſide, 49 feet $8\frac{1}{4}$ inches, = 52937 +	55)....(962,5
The depth of the receſs out of the great hall between theſe ſolids, is 33 feet $6\frac{1}{6}$ inches, = 35705,7	37)....(965
The diſtance between one of theſe ſolids and the neareſt angular ſolid, 32 feet $11\frac{5}{8}$ inches, = 35124,9	$36\frac{1}{2}$)....(962,3
The diſtance between the other of them and the other angular ſolid, 33 feet $1\frac{1}{3}$ inch, = 35276,57	$36\frac{1}{2}$)....(966,4
Entrance at one end of the hall, 36 feet $2\frac{1}{6}$ inches, = 38546,8	40)....(963,7
Entrance at the other end, 36 feet $1\frac{1}{4}$ inch, = 38465,4 — (twice)	40)....(961,6

Paſſage

Paſſage from the circular veſtibule, 30 feet $9\frac{1}{4}$ inches, = 32783,2 (twice) —— —— ——	34)...(964
Side entrance into the veſtibule, 20 feet $9\frac{2}{3}$ inches, = 22166,2 ——	23)...(963,7
A meaſure from 4 feet 6 inches to 4 feet $6\frac{1}{2}$ inches, repeated 12 times, the mean from all of them is 4 feet $6\frac{77}{288}$ inches, = 4818,03 ——	5)...(963,6
A meaſure from 5 feet 5 inches to 5 feet $5\frac{1}{4}$ inches, repeated 11 times, the mean from all is 5 feet $5\frac{2}{11}$ inches, = 5787,05 —— ——	6)...(964,5
A meaſure from 2 feet $8\frac{1}{2}$ inches to 2 feet $8\frac{5}{6}$ inches, repeated 4 times, the mean from all is 2 feet $8\frac{7}{12}$ inches, = 2892,86 —— ——	3)...(964,3
A multiple of 6 feet on the wall, where this meaſure of 3 feet is thrice repeated, is 43 feet $5\frac{1}{8}$ inches, = 46267,2 —— ——	48)...(963,9

The diameter of the circular veſtibule is too principal a part to be neglected. It anſwers to 65 Roman feet of 969,1 parts, which the other meaſures in this building will not come up to by probable diviſors. It anſwers likewiſe to $65\frac{1}{4}$ feet of 965,4 parts; but an odd palm in ſo large a meaſure is not probable. Therefore it ſeems to have been incorrectly executed, and perhaps this circle was deſcribed by a cord.

The

The ſhafts of the columns, each of one block, meaſure 35 feet $11\frac{5}{12}$ inches, = 38302,6 —— —— $39\frac{3}{4}$) ... (963,6

The whole column is 43 feet $6\frac{1}{6}$ inches, = 46359,7 —— —— 48) ... (965,8

Theſe are the principal meaſures in this building, and a mean taken from all of them, except the diameter of the veſtibule, gives a foot of 964,1 London parts.

But the three ſmall meaſures, of 5, 6, and 3 Roman feet (the firſt of which is the paſſus) are alone ſufficient to diſcover the meaſure of the foot by which this building was conſtructed. For, being ſo often repeated with ſo little variation, they muſt afford a mean very near the truth, if their diviſors be rightly aſſumed; and that theſe are ſo, can hardly be doubted. For, were the two largeſt to be encreaſed but by a ſingle digit, they would give different meaſures of the foot from each other; and that from the paſſus ſo encreaſed, would fall ſhort of 952 London parts; which is leſs than the leaſt of the antient foot-rules. On the other hand, were they diminiſhed by the ſame quantity, they would not agree together ſo nearly in the meaſure of the foot as they now do, nor give ſuch a meaſure of it as the reſt of the building will anſwer to by probable diviſors. Beſides, I have here reckoned only ſuch of theſe meaſures as Deſgodetz has figured on his plan; whereas they are much oftener repeated, and that not in the diameters of columns, or the members of an order, but in breaks, projections, and thickneſſes of walls, where a digit or two more or leſs could make no viſible

difference, provided the correſpondent parts were made to anſwer to each other: and it is highly improbable that ſuch odd meaſures as the paſſus wanting a digit, 6 feet wanting the ſame, and 3 feet wanting half a digit, ſhould be ſo often repeated in ſo large a building, and in places that do not ſeem to require any ſuch preciſe exactneſs. Therefore theſe meaſures muſt have been whole feet, and the diviſors are rightly aſſumed.

Their mean is 964,1 London parts, which exactly agrees with that found above from all the meaſures taken together.

The other remains of antiquity contained in this work are ſuch fragments as can be be of little uſe in the preſent enquiry.

CONCLUSION.

It appears from the meaſures of theſe buildings, that the Roman foot before the reign of Titus exceeded 970 parts in 1000 of the London foot, and in the reigns of Severus and Diocletian fell ſhort of 965.

Whether this difference proceded from any alteration in the ſtandard, or from a falſe meaſure of it being got into common uſe, either before the reign of Titus or after, is uncertain.

We have no account of any alteration made by law in the Roman ſtandards after the Plebiſcitum Silianum, quoted by Feſtus (9); but as great a difference as this might ariſe from their having been loſt or deſtroyed.

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(9) Under the words Publica pondera.

They were kept in the Capitol (1), and Rigaltius, from a paſſage in Hyginus, obſerves that the ſtandard of the foot was depoſited in the temple of Juno Moneta (2). Now the Capitol was burnt no leſs than three times; firſt in the civil war of Sylla (3); then again when Sabinus was beſieged in it by the troops of Vitellius (4); and the third time in that dreadful conflagration which happened in the reign of Titus (5). Whether the ſtandards were deſtroyed in the firſt of theſe fires is uncertain; but they could hardly eſcape the fury and confuſion of the ſecond, when, according to Pliny, the temple of Juno Moneta ſeems to have been burnt to the ground (6). And, if we may credit Xiphilin (whoſe account of the third is in ſome meaſure confirmed by Spartian), not only the temple of Jupiter Capitolinus, but thoſe adjoining to it, were burnt down in the laſt (7).

Veſpaſian

(1) This appears from the inſcription on Veſpaſian's congius, and from R. Fannius. And Jul. Capitolinus in Maximinis, mentions the amphora Capitolina.

(2) Hyginus de Limit. conſtituend. near the end, diſtinguiſhes the Roman foot by the name of Pes Monetalis. That this temple was in the Capitol, appears from Livy, lib. vii. c. 28. Cicero de Divin. lib. i. and Pliny, lib. xxxiv. cap. 7. Suidas (v. Μονητὰ) tells us the mint was kept here.

(3) Taciti Hiſt. lib. iii. This fire is likewiſe mentioned by Plutarch and Appian.

(4) Tacitus, ibid.

(5) Xiphilin in Tito. Sueton. in Domit. c. 5.

(6) Pliny, lib. xxxiv. c. 7. ſays, "Ætas noſtra vidit in Capitolio, priuſquam id noviſſime conflagravit, à Vitellianis incenſum, in cella Junonis canem ex ære, &c." And if the donatives were deſtroyed, it is not likely the ſtandards ſhould be ſaved in a ſcene of ſuch confuſion.

(7) Τόν τε νεὼν τῦ Διὸς τῦ Καπιτωλίυ, μετὰ τῶν συννάων αὐτῦ, κατέκαυσεν. Xiphilin in Tito. Spartian, enumerating the public

Veſpaſian rebuilt the Capitol after the ſecond conflagration, and reſtored the antient records from copies of them that were got abroad (8), and probably the ſtandards at the ſame time. The congius was reſtored by weight, according to the Plebiſcitum Silianum, as the inſcription on it teſtifies. The quadrantal was too cumberſome a veſſel for common uſe, to which the congius (like our gallon) was well adapted; ſo that I do not ſee what other purpoſe it could ſerve, but to adjuſt the congius to its capacity, and the foot to its ſide: but here we ſee the congius adjuſted by weight, and it is not very likely that a new quadrantal ſhould be made for no other end but to adjuſt the foot by, when ſo many copies of the old ſtandard were extant.

Therefore it is not improbable, that the ſtandard of the foot was at this time reſtored, without any regard to its relation to the quadrantal.

But as to the difference between the foot derived from the congius, and that found from other authorities, I muſt farther obſerve, that the correct adjuſtment of weights to meaſures, is a very difficult matter, even in this age, and in this kingdom, where workmanſhip is arrived at a high pitch of accuracy. And what errors rude workmanſhip is liable to, ſufficiently appears from the weights Pætus and Villalpandus have given of the congius. Therefore I ſee no reaſon to reject the teſtimonies of thoſe authors,

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buildings reſtored by Adrian, mentions moſt of thoſe that Xiphilin ſays were burnt in this fire. Domitian rebuilt the Capitol. Suet. in Domit. c. 5.

(8) Sueton. in Veſpaſ. c. 8.

who

who ſay that the cubic foot contained a quadrantal of wine; and as little to believe that theſe two ſtandards were ever truly adjuſted to each other.

But had the original ſtandard of the Roman foot been truly adjuſted to the quadrantal, and continued invariable from the time of its firſt eſtabliſhment, yet a falſe meaſure of it might at one time or other have got into common uſe at Rome, as well as a falſe meaſure of the French foot did at Paris; where in the year 1668, the maſon's foot was found to exceed the foot of the Chatelet by $\frac{5}{72}$ of a Paris inch (9), which is above $\frac{1}{14}$ of a London inch: and the unaccountable negligence which appears in the Roman coinage, gives ſufficient ground to ſuſpect they were not more accurate in their meaſures.

LXX. *A Deſcription of a metalline Thermometer*; *by* Keane Fitzgerald, *Eſq*; *F. R. S. Communicated by the Right Hon.* George *Earl of* Macclesfield, *Preſident of the Royal Society.*

Read May 22, 1760.

IT is univerſally allowed, that all bodies, whether ſolid or fluid, are expanded and contracted by heat and cold; and, as far as experiments of this kind have yet reached, it appears, that ſcarce any two bodies of different natures, or even of the ſame; are expanded, or contracted equally by the ſame degrees.

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(9) Picard, in the paper De Menſuris, quoted above.

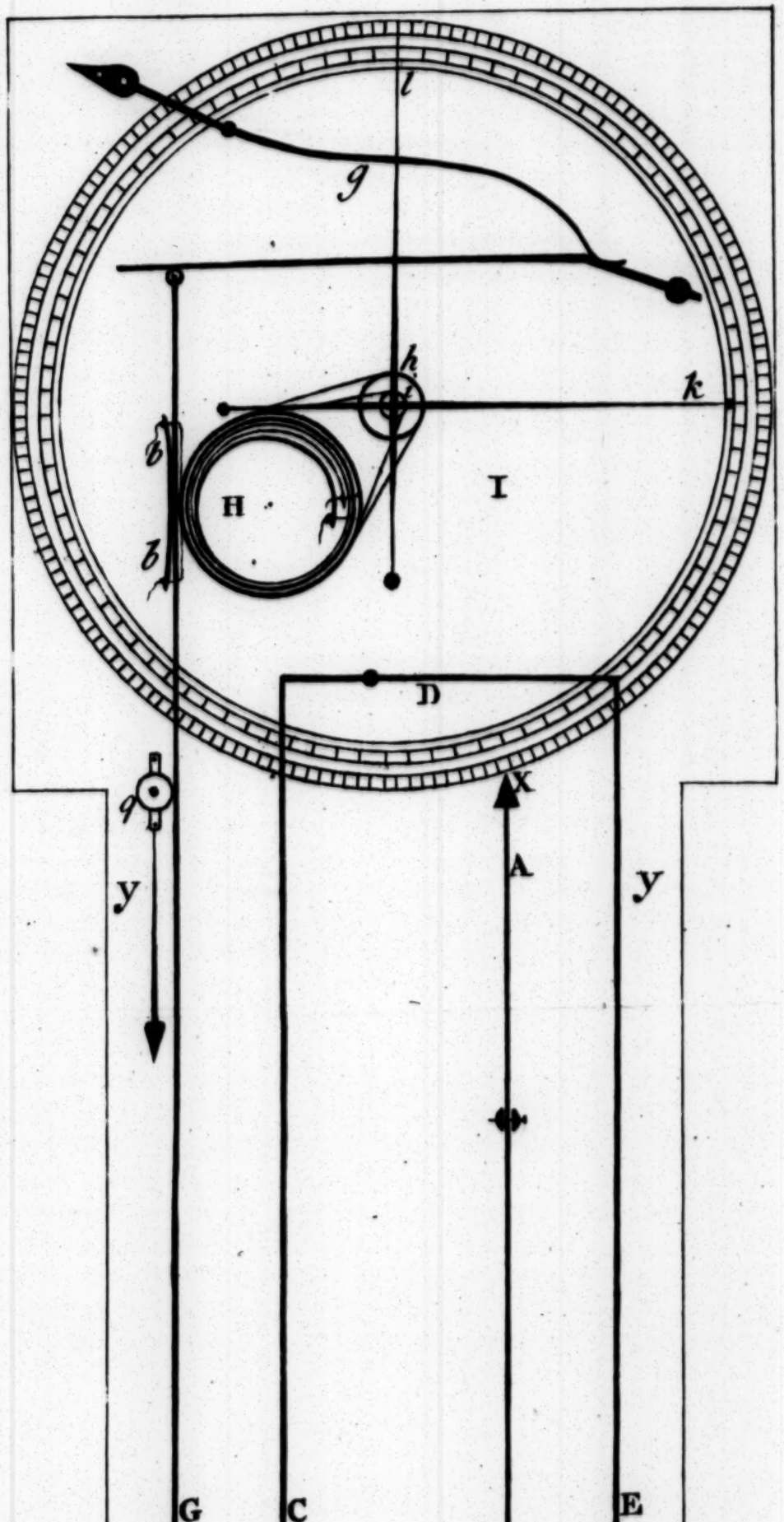
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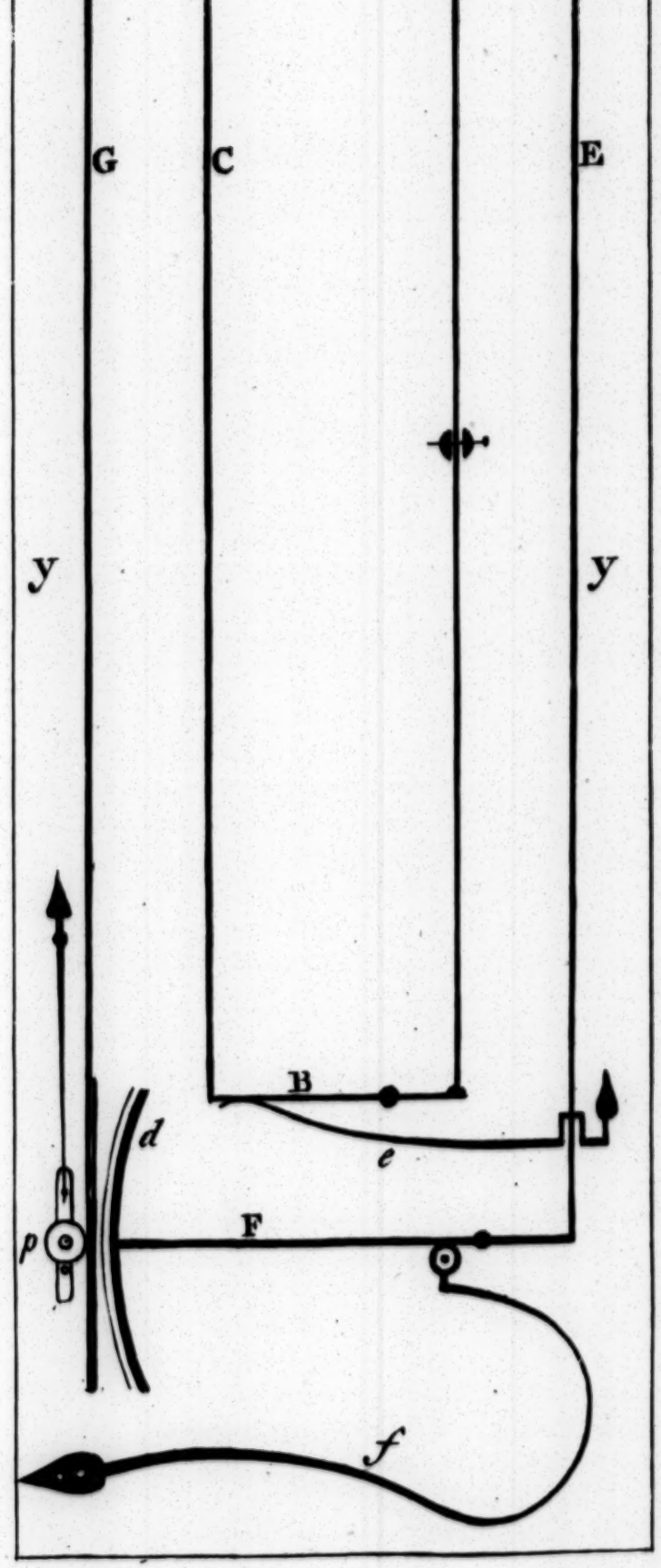
The advantages that may be derived from being able, by proper inſtruments, to aſcertain the degrees of natural and artificial heat and cold, with reſpect to philoſophical inquiries; and alſo to ſeveral uſeful, and common purpoſes in life; have induced many ingenious perſons to apply their thoughts that way; and endeavour to form ſome certain ſtandard, or gage, by which this may be obtained. The three principal bodies that have been made uſe of for this purpoſe; are air, alcohol, and mercury.

Mr. Boyle, the great promoter of experimental philoſophy, made a thermometer on the principle of air, which, to a certain degree of heat or cold, anſwered very minutely. Alcohol, or ſpirit of wine, has been more generally uſed; but has been found to loſe, in time, much of its expanding quality; and alſo to be frozen by an intenſe degree of cold. Mercury, as not deemed ſubject to theſe inconveniencies, has therefore been allowed the moſt proper for the purpoſe.

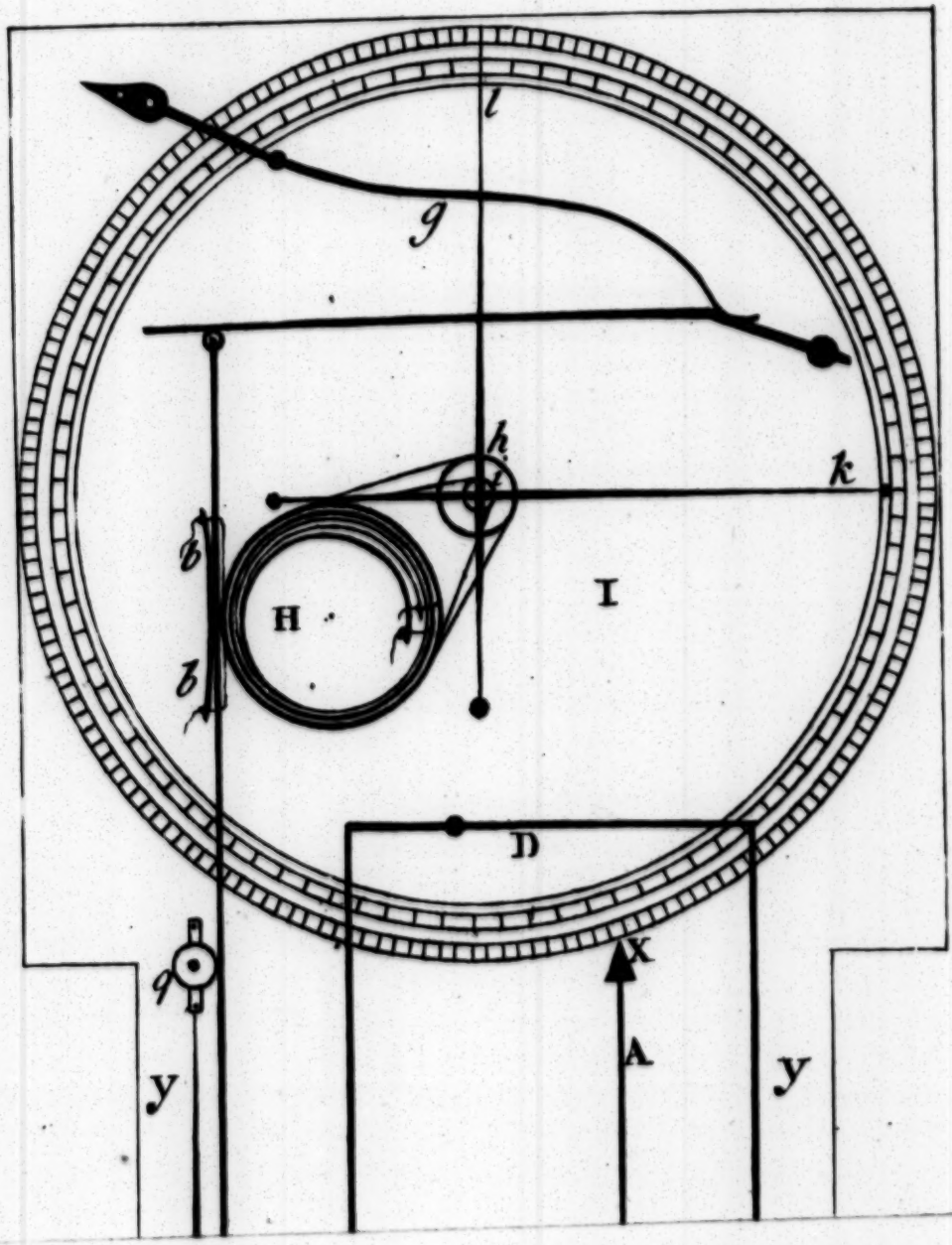
The learned Dr. Halley has obſerved, that mercury expands very ſenſibly at firſt, attaining very nearly the ſame height, ſome time before the water boils, in which it is placed; that it does on the ſtrongeſt boiling: whereas ſpirit of wine expands gradually, as the heat increaſes; but ſlower at firſt, and faſter after it is well warm; and, at a certain degree of heat, which wants much of boiling water, being almoſt tolerable to the touch, it boils vehemently; emitting bubbles, which, coming into the neck of the thermometer, lift the incumbent ſpirit to the top: from whence he concludes, that the

expanſion


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J. Mynde sc.


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expanſion of fluids graduated by equal parts, is not a ſufficient ſtandard of heat and cold.

Mr. Fahrenheit has ſince improved the mercurial thermometer to a great degree, and brought it to as much perfection as, perhaps, it will bear. He has remarked, that when the barometer ſhews a greater degree of preſſure of the atmoſphere; the ſame liquor will receive eight, or nine degrees of heat, more than when the barometer is at the loweſt. But whether this proceeds entirely from the liquor's receiving a greater degree of heat, by the preſſure of the atmoſphere, may be a matter of ſome doubt; as it ſeems, by comparing the mercurial, with other thermometers; to be affected, in ſome meaſure, by the preſſure of the atmoſphere, in all degrees of heat and cold.

The making of metalline thermometers has been hinted at by many; particularly by Mr. Smeaton, in his curious obſervations on the expanſion of metals, who recommends zink, or ſpelter, as moſt capable of expanſion, and fitteſt for the purpoſe. I have endeavoured to make one on this principle, which I take the liberty of laying before this Society, with a deſcription of its conſtruction, and an account of the few obſervations I have been as yet able to make on it. [*Vide Tab.* XVIII.]

It is compoſed of four metal bars, which act by a combination of levers, and pullies. The upper end of the bar A, which is 2 feet long, is oppoſed by a flat piece of metal *x*, ſcrewed faſt to a piece of deal board; *y*, *y*, *y*, *y*, and its lower end reſts on a ſmall hemiſphere, placed on the ſhorter arm of the lever B, which is fixed to the board *y*.

By

By the expanſion of the bar A, the ſhorter arm of the lever B is preſſed down: and as the ſpace deſcribed by each of the points of a lever is as the diſtance from the fulcrum; ſo the longer arm of the lever B, which is $2\frac{1}{2}$ as long as the other; is raiſed proportionably; and the ſecond bar C, 2 feet 2 inches long, the lower end of which is placed to the point of the lever B, by a ſmall pin, round which it is moveable; and the upper end, in like manner, to the ſhorter arm of the ſecond lever D; is raiſed accordingly; and thereby the longer arm of the lever D, which is $2\frac{3}{4}$ as long as the other, is proportionably preſſed down; to which is added the expanſion of the bar C, increaſed, in like manner, by the lever D.

The third bar E, 2 feet 4 inches long, is placed to the point of the longer arm of the lever D, and the ſhorter arm of the third lever F, which it preſſes down accordingly; and the longer arm of this lever, which is four times as long as the other, is thereby proportionably raiſed; to which is alſo added the expanſion of the third bar E, increaſed by the lever F.

There is an arch-head *d*, fixed to the point of the longer arm of the lever F, to which the fourth bar G, 3 feet long, is fixed by two chains, which raiſe and lower it, as that arm riſes or falls; and, at the diſtance of 2 feet 6 inches, there is a pulley H, 3 inches diameter. There are two cords fixed to this pulley, ſurrounding it different ways, one leading upwards, the other downwards, which paſs through the bar G, and are fixed to two ſprings *b*, *b*, which keep them equally tight; and the bar cloſe to the

the pulley H, which is turned different ways, as the bar G is moved upwards or downwards.

There is a pulley *p* fixed on the other ſide of the bar G, oppoſite the arch-head of the lever F, on which the bar rolls; and which, by the help of a ſlender ſpring, keeps it cloſe to the arch-head; towards the upper end, there is another pulley *q*, on which it alſo moves.

The expanſion of the bar G, from the place where it is fixed to the arch-head *d*, to the place where it is fixed by the ſtrings to the pulley H, is to be added to the expanſion of the other three bars, increaſed by the ſeveral levers.

There are two ſmall pullies *h* and *i*, the pulley *h* one inch diameter, and the pulley *i* a quarter of an inch diameter, whoſe axis paſſes through that of the pulley *h*, like the hands of a clock; and on theſe are placed, in like manner, two indexes *k* and *l*, which move round the dial I, that is 12 inches diameter. Theſe pullies are ſurrounded by cords, which alſo ſurround, and are faſtened to ſprings within the pulley H. The index *l*, which may be called the minute-hand, is moved 4 times round for once the index *k* is moved round; the index *l* ranges at its extreme point 48 times as much as the bar G riſes or falls; and the index *k* 12 times.

As the powers of theſe levers are all reverſed, with reſpect to the preſſure by the expanſion of the bars; there are two counterballance ſprings *e* and *f*, placed under the longer arms of the levers B and D, which help to raiſe the bars eaſily; and there is another ſpring *g*, acting on a ſmall roller, placed on the top of the

bar G, which returns all the bars on their contraction.

On the dial-plate I, there are three circles deſcribed. The inward circle is divided into 240°, according to Fahrenheit's ſcale. The middle circle is divided into 360°, which is intended to mark the expanſion of different metals. The outward circle is divided into 1080 parts, to which the longer index *l* points, which marks 18 diviſions for each degree of the inward circle; and 12 for each degree of the middle circle, pointed to by the index *k*.

The quarter diviſions of the dial are marked I, II, III, IIII, to ſhew, by the index *k*, how often the index *l* has gone round.

There is a ſcrew-hole towards the point, in the index *k*, to fix a ſmall pencil in; which is moved by the index on a circle of wood, or ivory, and marks the higheſt and loweſt degrees it has pointed to, for any time; which ſaves the continual examination requiſite to form obſervations of this kind; and the mark of the pencil is eaſily rubbed out, when any new obſervation is intended.

The firſt bar A of this inſtrument may readily be taken out, and replaced; or any other bar of equal length and thickneſs applied; by which means, the inſtrument may be uſed as a pyrometer, to meaſure the expanſion of any bar 2 feet long, with great exactneſs, to the 73,840th part of an inch *per* foot; and, as a thermometer, it marks the alterations in the temperature of the air, much more ſenſibly than any other inſtrument for the purpoſe; the range of the minute index *l* being 74 inches, by the common degrees of heat and cold of this climate.

If

If the greateſt expanſion, from freezing to boiling water, of the metal bars to be made uſe of in an inſtrument of this kind, is certainly known; it may be made to mark any minute degree intended. But to anſwer a calculation of this kind; the levers, and pullies, muſt be made with the greateſt exactneſs; which I could not prevail on the workman that made mine to be ſo careful of as I could wiſh. It is eaſy, however, to come to a certain knowlege of the expanſion, mechanically. I took a thick book, and preſſing the leaves between my finger and thumb gently; meaſured off an inch exactly, with a pair of compaſſes, from the 1ſt page, which made 568 pages. I cut off ſeveral ſlips from different leaves, each of which I placed between two bits of watch-ſpring, which were marked, in order to place them in the ſame poſition exactly, on each trial. On examining theſe with the inſtrument, I found but few of an exact thickneſs; but that, on a medium, each leaf raiſed the minute index 130 diviſions. So that the number of pages, each leaf of which is the 284th part of an inch, multiplied by the number of diviſions it raiſes the index; ſhews, that each diviſion marks the 73,840th part of an inch expanſion *per* foot of the bar A, which is 2 feet long.

This bar is made of ſpelter, which is of ſo brittle a nature, as makes it difficult to file one of that length without breaking. If it is not well caſt, it will be extremely hollow; as was the caſe of a bar I made uſe of, which did not expand near as much, nor as uniformly, as another of the ſame metal, and contracted much more readily; which I could not well account for, until it broke by accident; when

it appeared full of large cavities on the inſide, though the outſide ſeemed as fair as any other. Some of theſe cavities were above an inch long, and the ſurrounding metal, after filing, not above the thickneſs of a card. I would therefore recommend the weighing bars of this metal, in order to obſerve the difference of expanſion.

The other bars of this inſtrument, as the founder informs me, are made of 18 parts ſpelter, and 2 of copper. I ſhould imagine, that there is a greater proportion of copper than he owns, from the appearance of the metal, and the diſproportion in its expanſion, to what I ſuppoſe 2 parts in 20 might occaſion.

Since this inſtrument has been made, I found, on looking over the 10th volume of Martin's Abridgment of the Philoſophical Tranſactions; that Dr. Mortimer had, in 1735, given the Royal Society a deſcription and drawing of an inſtrument he invented for the purpoſe; and that Mr. Johnſon had alſo given a drawing of another, invented by Mr. Fothringham. Although theſe are of a very different conſtruction, yet, as they were formed on the ſame principles, I ought, in juſtice to theſe gentlemen, to mention them.

I have made what obſervations I could on this inſtrument, ſince the ſhort time it is made; by comparing it with a Fahrenheit's, and a ſpirit thermometer; and find, that it keeps at a medium between both; not riſing at firſt ſo quick as the mercury, and ſomewhat quicker than the ſpirit. On placing them together in the ſun, when its heat became intenſe, it roſe at laſt faſter than the mercury, and not ſo faſt

as

as the ſpirit; and continued to riſe for ſome time after the others became ſtationary.

I electrified the bars of the inſtrument, to ſee if the electrical fire could produce any degree of heat ſufficient to expand them; which, on the firſt trial, it ſeemed to do, by the minute index riſing 6 diviſions in a ſhort time. But as I had ſome reaſon to imagine; that this appearance proceeded rather from an increaſe of heat, occaſioned by two gentlemen being in the room with me, when I made the experiment; I repeated it alone the next day, leaving the door open at the time, and could not perceive the minute index to riſe above one diviſion; which I attribute rather to the warmth that my being in the room had occaſioned.

I tried the expanſion of a few metal bars, from artificial freezing, with pounded ice, and water that it diſſolved into; upon which was poured half an ounce of ſpirit of tartar, in which Fahrenheit's thermometer deſcended to within one degree only of the freezing point: to boiling water, in which it roſe to 211°, though the water did but ſcarce boil, for want of a ſufficient number of lamps. The barometer ſtood at 30 inches, and the natural heat of the weather at 60° of Fahrenheit.

	Diviſions.
* A bar of ſpelter 2 feet long, marked by the minute index — — — — — —	1570
Spelter 18 parts, and copper 2 parts, according to the founder's account — — —	1150
Braſs — — — — — — — —	1120

* This was the bar, found, on breaking, to be hollow.

Iron

	Divisions.
Iron — — — — — — — —	785
Steel — — — — — — — —	695

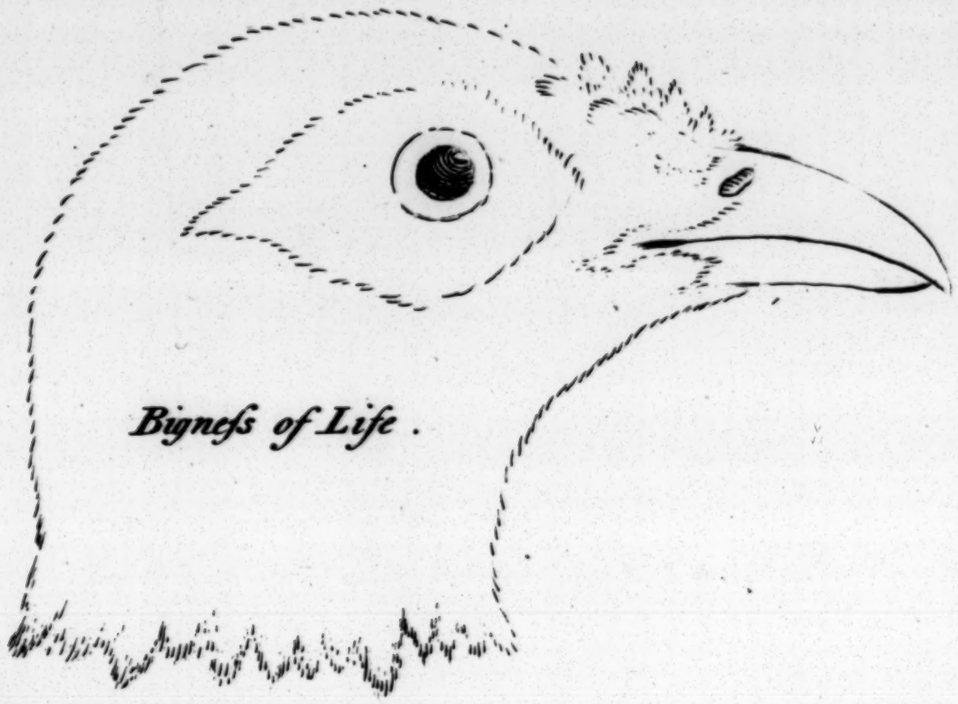
Note. Each diviſion marks the 73,840th part of an inch expanſion *per* foot.

I find that theſe come ſomewhat near Mr. Smeaton's table. They are far from being as complete as I wiſh; but hope to be able to give a more accurate account hereafter; as it certainly requires the natural cold to be added to the artificial, in order to get at the laſt degree of the contraction of metals, which can be come at in this climate.

If an inſtrument of this kind ſhould be deemed worthy the attention of the curious, there is no doubt, but it may be carried to a much greater degree of perfection than this, which is but a rude beginning. The friction is little or none, as the ſpring at the top of the fourth bar, keeps the ſeveral levers and bars in the ſame poſition on their axes, whether riſing or falling. It may be made much more eaſily, and at a leſs expence, by making uſe of counterballance weights, inſtead of ſprings; and by a cord paſſing over a pulley placed on the top of the fourth bar, with a weight fixed to it, which returns the bars, on their contraction.

I have one making in this manner. The firſt bar of which is 3 feet long, and by a ſlider, which is fixed by a ſcrew, it will meaſure the expanſion of any bar, within that length. The powers of the levers are alſo greater; and the minute hand goes 12 times round, for once the other goes round; by which it marks the 300,000 part of an inch expanſion *per* foot, and ranges 216 inches, by the common degrees of heat and cold in this climate.

Mercury



A Bird ſuppoſed to be bred between a Turkey and a Pheaſant.

J. Mynde ſc.

Mercury cannot be uſeful in trying any degrees of heat above what makes it boil: and it appears by Dr. Hinſell's account of the experiments lately made at Peterſbourg, that it may be frozen by extreme cold; which makes it unfit for aſcertaining the extreme degrees of either.

An inſtrument of this kind might be made by the help of a tin caſing, to come between the bars and the wood, to receive a bar of iron or ſteel, heated to any degree within that of its melting; without any detriment to it. And any kind of metal bar will certainly bear the moſt extreme degree of cold, and probably contract proportionably. I ſhould imagine it might alſo be made very uſeful in trying minutely the expanſion of different kinds of metals, fit for making compound pendulums; and the exactneſs of the pendulum, when made, might likewiſe be tried, by artificial freezing, or in hard froſt, and by boiling it in water.

LXXI. *An Account of a Bird ſuppoſed to be bred between a Turkey and Pheaſant*; *by Mr.* George Edwards, *F. R. S.*

To the Rev. Dr. Birch, *Secretary to the Royal Society.*

SIR,

Read May 22, 1760. HAving in my hands a bird, that, I believe, may be a curious and entertaining ſubject to the Royal Society, I preſume, by your favour, to lay it before them [*Vide Tab.* XIX.], with its deſcription, and what other account of it I could

could procure. I received it from the very curious and worthy Henry Seymer, Eſq; of Handford, near Blandford, Dorſetſhire, with his letter, dated April 9, 1760: wherein he ſays,—" I have taken the firſt " ſafe opportunity of ſending the two birds. The " large one [the ſubject lying before the Society], I " verily believe, is an accidental croſs, as we ſportſ- " men term it, between a pheaſant and turkey. " You are to be told, that, when the bird was juſt " killed, the ſkin round the eyes was of a pale red- " lead colour, and the eyes like a turkey's. As I live " near the wood where they were found, I took " great pains to get another of them, but was never " ſo lucky as to find one. There were three at firſt, " all of which, I believe, are now deſtroyed. You " are heartily welcome to it; but I beg the favour of a " painting of it by your own hand; this I ſhould " eſteem in the higheſt degree. I am ſenſible it re- " quires a nice hand to copy the feathers of the back " and tail; but your judgment and experience have " long rendered things, extremely difficult to others, " eaſy to you."

In the ſame letter, Mr. Seymer informs me, that the bird was ſhot in October laſt, and he ſuppoſes it to be the production of the ſpring or ſummer immediately preceding, it having not, as he thinks, molted off all its firſt or chicken feathers, as they are called; and that it would have required a ſomewhat different deſcription, had it been older. My deſcription of it, in its preſent ſtate, is as follows.

It is of a middle ſize between a pheaſant and a turkey-hen, and ſhaped pretty much like a turkey; the bill, legs, and feet, are black, and ſhaped like a turkey's;

turkey's; it hath a broad ſpace of bare ſkin round the eyes, which, when the bird was living, was of a pale red-lead colour; the eyes like thoſe of a turkey; the head, and half the neck, is covered with very ſhort feathers, of a whitiſh clay colour, with tranſverſe duſky bars, though the throat and fore-part of the neck are wholly of a light clay colour. Theſe ſhort feathers occupy the head and that part of the neck, which is naturally void of feathers in turkeys. On the lower part of the neck, the breaſt, and belly, the feathers are much longer, and of a black colour, with a purple and changeable gloſs. The thighs and legs, on their fore-part, a little below the knees, are covered with feathers tranſverſely barred with clay colour and black. The back, covert feathers of the wings and tail, are of a mixed colour, in very fine tranſverſe lines of brown and black, though ſome of the coverts of the wings and tail have larger tranſverſe bars of the aboveſaid colours; the greater quills are duſky, or black, powdered with ſmall clay-coloured ſpots; the inner coverts of the wings have white tips, which hide their bottoms, that are duſky. I counted ſixteen feathers in the tail, the outer ones ſhorter by two inches than the middlemoſt; their colour is compoſed of brown and black, mixed tranſverſely, like thoſe on the back, though they are more duſky toward their tips; the very tips being of a bright brown: the outer borders of the ſide feathers of the tail are of a bay colour; the covert feathers beneath the tail are of an orange colour, croſſed with black; about the vent, the feathers are white, with duſky ſpots. The whole upper ſide nearly reſembles that of a hen-pheaſant, but darker coloured. The feathers of the body

are all double; that is, two diſtinct feathers proceeding from one ſtem; the outer large, and of a firm texture; the inner ſmaller, and altogether downy.

Whether this bird be produced from a turkey-hen and a cock-pheaſant, or from a turkey-cock and hen-pheaſant, no one knows. I ſhould think it rather from a hen-turkey and cock-pheaſant; becauſe their diſparity in ſize is not near ſo great, as between the turkey-cock and hen-pheaſant, Though the ſuppoſition, that this bird is from an egg laid by a hen-turkey trodden by a cock-pheaſant, is attended with a difficulty not eaſily reconciled; for it is not probable, that a hen-turkey, a domeſtic fowl, ſhould betake herſelf to the woods, and bring up her brood wild, and unobſerved; which is contrary to the habit of turkeys in our country, where they are not originally natives. Why theſe mixed generations ſo rarely happen, is, I believe, becauſe nature has fixed the inclination of every diſtinct ſpecies to the contrary ſex of its own identical ſpecies, from which, in a wild and natural ſtate, it will hardly ever ſtray. The reaſon of the mixtures, that we meet with, contrary to the ordinary courſe of generation, may proceed from ſome hinderance of the male's meeting with his proper female, or female with male, at the ſeaſons, when they are by nature appointed to propagate their ſpecies, which rarely happens; for, in a wild ſtate of nature, moſt animals are numerous, and, at their breeding ſeaſons, eaſily meet with males or females of their own ſpecies. Diſappointments of what they naturally ſeek, and accidental meetings of different ſpecies, near of kin to each other, cauſe theſe unnatural conjunctions, which produce uncommon

mixed ſpecies of animals. I believe, that two ſpecies widely different from each other, as water-fowl and land-birds, *&c.* cannot poſſibly conjoin, ſo as to produce a living mixed offſpring. I have been informed, and believe it may be true, that a mixed ſpecies has been produced between our common poultry, and partridges that harbour near farm-yards.

I am, SIR,

Your moſt humble

and obedient ſervant,

London, May 22, 1760. Geo. Edwards.

LXXII. *An Account of a late Diſcovery of Aſbeſtos in* France: *In a Letter to the Rev.* Tho. Birch, *D. D. Secretary to the Royal Society, from Mr.* Turberville Needham, *F. R. S.*

Reverend Sir,

Read June 5, 1760. I Have little to add in the way of literature, except a ſingular diſcovery accidentally made, lately, in one of the French provinces, of the nature of the aſbeſtos, or amianthus. The proprietor of a certain forge, upon taking down his furnaces to repair them, found a great quantity of this ſubſtance at the bottom. It anſwered effectually all the common uſes of the native amianthus, either

manufactured into linen, or paper. In ſhort, upon a progreſs in this inquiry, he finds, that both this, which he obtained from his forge, and the native aſbeſtos, is nothing more, as he terms it, than calcined iron, deprived of the phlogiſtic; and that, by uniting the phlogiſtic, either with this, or the foſſile amianthus, he can reſtore it, at any time, to its primitive ſtate of iron. Does not this, with the diſcovery of lava, pummice-ſtones, iron in a perfect ſtate, and, in fine, many other traces of fire obſerved in moſt of the mountains, particularly in all the great chains, and remarkably in all thoſe under the equator, which are the higheſt upon the globe, ſeem to indicate, that the dry land, with all its eminencies, was originally raiſed out of the waters, by the force of ſubterraneous fire?

I am,

With great reſpect and eſteem,

Reverend Sir,

Your moſt obedient,

humble ſervant,

Paris, May 17, 1760. T. Needham.

LXXIII. *An*

LXXIII. *An Account of the hot Baths of* Vinadio, *in the Province of* Coni *in* Piedmont; *with the State of the Weather at* Turin *in the Year* 1759: *In an Extract of a Letter from Dr.* Joſeph Bruni, *F. R. S. to Mr.* Henry Baker, *F. R. S.*

Dear Sir, Turin, Feb. 24, 1760.

Read June 5, 1760. THE warm baths, which have been ſo very ſerviceable to the Chevalier Oſſorio, run through the rocks, near the village called Vinadio, in the province of Coni.

The water is very clear, and ſo warm, one cannot bear the hand in it: the contents are ſulphur diffuſed through it, and ſome ſalt almoſt like common ſalt. By evaporation, you get five, ſometimes ſix grains, from a pound of water. I ſend you a ſmall quantity of the ſalt, gathered from the ſtones by which the water runs, before the riſing of the ſun, for in the day-time it is not found upon them, except in winter. Where theſe waters run, they depoſit oily particles, which, by degrees, join together, and form a ſoft, ſpongious, greeniſh-yellow ſubſtance, one inch almoſt in thickneſs, which is called *muffa*: this, when dry, is wrinkled, takes fire, crackles, and gives the ſmell of brimſtone, and when entirely burnt, leaves a black aſh behind. I immerſed a piece of ſilver in theſe waters, which, in a few minutes, became black. Their taſte is neither ſalt nor acid, but diſagreeable. The *muffa* left for two days in common water, ſwells ſix times thicker than it was, ſhrinks, and throws up oily particles on the ſurface of the water.

The

The ſalt does not ferment with acids. If you diſſolve it in common water, and mix it with ſyrup of violets, it gives ſome appearance of a green colour: the ſame water poured on a ſolution of ſilver, it ſoon throws down a white ſediment. Some ſay theſe waters contain nitre, and particles of other bodies; but this has not been demonſtrated.

The diſorder of the Chevalier Oſſorio, for which I adviſed him to the uſe of theſe baths, was, that he had loſt the feeling of his fingers, had a weakneſs in his hands, and alſo in his legs, inſomuch, that ſometimes he could not walk in a ſtrait line, but tottered from ſide to ſide. He could not extend his toes, and the ſoles of his feet feeled, according to his own expreſſion, as if hard ſtrings were drawn acroſs them. He tried many medicines to no purpoſe; but is now perfectly free from the above complaints, by the uſe of theſe warm baths; of which he is ſo fully convinced, that he intends to go to them again next ſummer, notwithſtanding at that time of year the heat is there exceſſive.

He bathed in theſe waters forty times, when the ſtomach was empty, in a morning; and ſtayed in them, at firſt, half an hour, but lengthened the time, gradually, at laſt, to a full hour. After each bathing, he was dried with cloths, and put into a warm bed, where a plentiful ſweat came on for about half an hour; during which, the pulſe beat like that in an high fever, but became quieter, as the ſweating abated. When the ſweating was almoſt over, and the pulſe quite regular, he was dried again with cloths, his ſhirt was changed, and, ſitting up in bed, he was refreſhed with a glaſs of ſtrong wine, and a

piece

piece of bread. After this, he roſe, and dreſſed, and took a gentle walk.

I obſerved, while I was there with the Chevalier Oſſorio, that no patient, who came thither, received the leaſt prejudice by the waters, though all did not receive alike benefit for their reſpective diſorders. But one gentleman in particular, who came paralitic, in the whole inferior extremities of his body, occaſioned by hard drinking, I ſaw ſo far relieved as to walk without help. I alſo ſaw others cured of diſorders in the ſkin, and relieved in rheumatic and many other complaints.

Several perſons drank the waters of a neighbouring ſpring, which is lukewarm, but I cannot ſay to any great advantage: the cures I obſerved were by bathing only. Where there is a partial diſorder, the waters of Vinadio are pumped upon the diſtempered part.

Olaus Borrichius mentions a remarkable cure effected by theſe waters. *Vide Medic. Septentrion. Collat.* tom. II. p. 855.

I ſend you alſo an account of the weather in this country, in the laſt year 1759. It ſhews the higheſt and loweſt ſtate of the barometer and thermometer in every month.

The firſt cypher denotes the day.

Mat. ſignifies the morning; *D. M.* the afternoon; the following cypher the hour; and the laſt cypher the inches, and parts of inches, divided in 12.

In the thermometer, o denotes the freezing point; and the laſt cypher the degrees above or under the ſame. The thermometer is made according to that of Monſ. De Reaumur.

BARO-

BAROMETRO 1759.

Months.	Days.	Morn. or Aft.	Hours.	Inches.	Parts.
Gennaio	3.	D. M.	2	27	1½
	17.	Mat.	7½	27	10
Febbraio	25.	Mat.	7½	27	1
	14.	Mat.	7½	27	10
		D. M.	2	27	10
	15.	Mat.	7½	27	10
Marzo	11.	Mat.	7	26	11
	15.	Mat.	7	27	6
Aprile	15.	D. M.	4	26	11
	16.	Mat.	7	26	11
	17.	Mat.	7	27	6
Maggio	4.	D. M.	3	26	11
	20.	Mat.	7	27	8
Giugno	30.	D. M.	3	27	1
	20.	Mat.	6	27	7½
Guglio	4.	Mat.	6	27	0½
	9.	Mat.	6	27	8
	10.	Mat.	6	27	8
Agosto	21.	D. M.	3	27	2
	5.	Mat.	6	27	7
Settembre	22.	D. M.	2½	27	3
	23.	Mat.	6	27	3
		D. M.	2½	27	3
	17.	Mat.	2½	27	7
	18.	Mat.	6	27	7
Ottobre	28.	Mat.	7	27	2
		D. M.	2½	27	2
	29.	Mat.	7	27	8½
Novembre	22.	D. M.	2	26	9
	23.	Mat.	6	27	10½
		D. M.	2	27	10½
Dicembre	3.	D. M.	2	26	11½
	24.	Mat.	7½	26	11½
		D. M.	2	26	11½
	2.	Mat.	7½	27	6

TERMO-

TERMOMETRO 1759.

Months.	Days.	M. or A.	Hours.	Degrees.	
Gennaio	7. 26. 31.	Mat.	7½	0	
	12, 13, 14.	D.M.	2	6	
Febbraio	1. 4, 5.	Mat.	7½	0	
	17. 22.	D.M.	2	8½	
Marzo	14, 15.	Mat.	7	1	
	28.	D.M.	2½	13	
Aprile	1.	Mat.	7	4	
	14. 23, 24.	D.M.	2½	17½	
Maggio	5. 19.	Mat.	6½	9	
	13. 15, 16.	D.M.	3	21	
Giugno	4. 6.	Mat.	6	11	
	19.	D.M.	3½	26½	
Guglio	2.	Mat.	6	12½	
	22.	D.M.	3½	28½	
Agosto	24.	Mat.	6	12½	
	6. 18.	D.M.	3½	26	
Settembre	22, 23.	Mat.	6½	11	
	9, 10. 12.	D.M.	3	24	
Ottobre	6. 25, 26, 30.	Mat.	7	9	
	1. 3. 7, 8, 9. 21. 23.	D.M.	2½	17	
Novembre	26.	Mat.	7½	4½	Sotto lo zero.
	19. 26. 29.	D.M.	2	1	
Dicembre	28.	Mat.	7½	0	
	29.	D.M.	2	2½	

LXXIV. *An Account of a Specimen of the Labour of a Kind of Bees, which lay up their Young in Caſes of Leaves, which they bury in rotten Wood: In a Letter to* Daniel Wray, *Eſq*; *F. R. S. from Sir* Francis Eyles Styles, *Bart. F. R. S.*

SIR, Batterſea, June 11, 1760.

Read June 12, 1760. I Send you by the bearer a ſpecimen of of the labour of a kind of bees, who lay up their young in caſes of leaves, which they bury in rotten wood. I make no doubt but they are the ſame, which are deſcribed in the Tranſactions of the Royal Society, by Sir Edmond King, Mr. Francis Willoughby, and Dr. Liſter. See Lowthorp's abridgment, vol. ii. p. 772, *&* *ſeq.*

Monſ. Reaumur, in his Hiſtory of Inſects, tom. vi. p. 39. deſcribes a kind of bees, which he calls *percebois* (wood-borers). But theſe, he tells us, form no caſes for their young, but lay them in the holes they make in the wood, without other covering, except artificial floors, which they make of the ſame wood, to divide the length of the holes into ſeparate lodgements, each of which contains a ſingle bee. In p. 97. of the ſame volume, he deſcribes another kind of bees, which he calls *coupeſeuilles* (leaf-cutters); and the deſcription, which he gives of their work, ſeems to anſwer to the ſpecimen I ſend you. But he tells us, that all the ſpecimens he had ever found, or been able to collect, of their labours, were taken out of

of the ground; and ſeems unwilling to rely on what he met with in Ray, concerning the depoſiting theſe caſes of leaves in rotten willows. Monſ. Reaumur's words are as follows.

" Entre les coupeuſes, et même entre les coupeuſes
" de feuilles de Roſier, il y en a, qui ſçavent placer
" leurs étuis dans des lieux, où ils peuvent ſe con-
" ſerver ſains plus longtemps, ſi, comme Ray le rap-
" porte, mais ce qu'il a negligé de dire qu'il avoit
" vû, les ètuis de feuilles, qu'il a décrits, avoient été
" réellement tirés de trous percés dans du Bois de
" Saule pourri. Pour moi je n'en ai encore vû
" qu'en terre, et ceux qui me ſont venus de divers
" endroits, ont tous été tirés de terre."

As Monſ. Reaumur queſtions the truth of the account he met with, it may, perhaps, be agreeable to the Royal Society, to have a ſight of the work in queſtion; and, if you think ſo, I ſhall be obliged to you, if you will take the trouble of ſhewing it to them, at their next meeting. I ſhould have told you, that the ſpecimen was found in ſome park pales near Windſor, the latter end of laſt ſummer, by ſome workmen, who brought it to Mr. Lee, nurſery-gardener at Hammerſmith, from whom I received it. One of the bees hatched, and crawled from his caſe, under my eye, on Whitſunday laſt; and, by an empty caſe I ſaw, that was broke open much in the ſame manner, I imagine another had hatched, and flown away a little before. The remainder, I preſume, will not come to life, as I obſerve, that ſome foreign inſect has made its way into ſome of the caſes; and others may have been chilled in the winter, by the fracture of the wood, in which they were incloſed. I have

deſtroyed ſome of the caſes, in examining them; but there are enough left in their original ſituation, to ſhew how they are contrived and diſpoſed.

I am, SIR,

Your moſt obedient

humble ſervant,

F. H. Eyles Styles.

LXXV. *An Account of a Caſe of a luxated Thigh Bone reduced; by Mr.* Charles Young, *Surgeon, at* Plymouth: *Communicated by* John Huxham, *M. D. F. R. S.*

Read June 12, 1760.

AS John Down, a middle ſized man, aged about forty, was, on the 21ſt Auguſt 1759, harneſſing his maſter's horſes, they ſuddenly took fright, and ran away with the chaiſe. He had his back towards the chaiſe, the wheel of which, as it rolled very ſwiftly along, ſtruck him on the upper and hinder part of the right thigh. He fell to the ground, and was unable to riſe again, and complained immediately of a violent pain in his right hip. I came to him ſoon after the accident, and cauſed him to be put to bed; when, on examination, I found his only complaint was the violent pain about the articulation of the femur with the iſchium, which was increaſed by any even the leaſt motion of the limb.

limb. The toe was turned in toward the heel of the left foot, and the heel of courſe outward; and the whole limb, from the head of the femur to the toes end, diſtorted in proportion.

I thought it was very evident, at firſt ſight, that there muſt be either a fracture of the femur, or a diſlocation of its articulation with the iſchium. The former, I think, would have been eaſily diſcoverable. But as by laying my hand on the great trochanter, while an aſſiſtant turned the foot inward and outward alternately, I could perceive, that the motion of the great trochanter correſponded exactly to the motion of the loweſt end of the femur, I concluded, that, had there been a fracture, it muſt have been between the great trochanter and the head of the bone. And, had this been the caſe, I expected to have been able to diſcover it, by the grating, that is always to be felt, when the two broken ends of a bone are moved againſt one another. But no ſuch thing being perceivable, and yet the limb ſo much diſtorted, and the pain ſo violent, and confined to the parts about the joint, I took it for granted, and pronounced the caſe to be a diſlocation of the femur; and conſequently endeavoured to reduce it, by the uſual method of extenſion. To this end, two men extended the limb, by pulling on napkins tied round the ancle, while others counteracted them, by pulling on a ſheet paſſed between his legs, and ſecured at the bed's head, turning the foot outward as they made the extenſion. This gave him great pain; but the limb ſoon became, in every reſpect, parallel to the other. It appeared as long, and, on laying it down on the bed, the great toes and heels of both

feet

feet lay exactly in the ſame poſition; and the only difference, which I could perceive in the two limbs, was, that there was a little flatneſs about the hip of the right ſide. In ſhort, the difference between the two limbs was ſo little, that I began to think I had been miſtaken in my opinion of a diſlocation (for it was evident there was no reduction made by the extenſion, for that could not but have been perceived, both by the patient and myſelf), and that the diſtortion of the limb was owing to nothing elſe than an involuntary contraction of the muſcles, occaſioned by the violence of the blow. I therefore bled him, confined him to lie on his back, and charged him to move as little as poſſible; imagining, that reſt would be his moſt effectual remedy. He continued in much pain for ſome days; but by degrees grew tolerably eaſy, except when the limb was moved; and at about twelve days after the accident, he could ſuffer the limb to be lifted to and fro gently, with little or no pain at all. Notwithſtanding which, he could no more lift it of himſelf than at firſt, when it was much more painful. This embarraſſed me a good deal. I was convinced there was no fracture of the limb, in any part; and I thought, from the circumſtances above related, there could hardly be a diſlocation. I therefore deſired the opinion of two other ſurgeons, who, on ſeeing the poſition of the limb, and inquiring into all the circumſtances, which did and had attended it, agreed with me in opinion, that it was no fracture, and that it was equally unlikely, under theſe circumſtances, there ſhould be any diſlocation. For the right leg, when placed by the ſide of the other leg, as I have ſaid already, was exactly

parallel to it, and continued ſo, unleſs the patient, either when aſleep or at any other time, moved his body, ſo as to drag his leg: in that caſe, the toe was always found inclining inwards, and the heel outward; but never ſo, but that it might be replaced, without the leaſt difficulty or pain to him, but juſt ſo as one might have done by a limb that was paralytic. We therefore recommended reſt, hoping that farther reſt and time would recover the perfect eaſe and ſtrength of the limb. But ſome days paſſing without any alteration, I gave him a ſtrong purgative, and repeated it every other or every third day, for ſeveral times, in order to reduce the muſcles, that I might the more plainly feel any thing through them. For though he was, in point of height, but middle ſized, he was pretty fleſhy, and the glutæi muſcles conſequently too thick to ſuffer any thing to be felt with any degree of diſtinctneſs through them. This anſwered my expectation fully; for, by repeating the purgative often, and at ſhort intervals, his living ſparingly, and being confined to his bed, he became much emaciated, inſomuch, that the head of the femur was plainly felt through the muſcles, diſlocated backward, and lying in the ſpace between the os iſchium and os ſacrum.

Of this I acquainted the gentlemen, who had examined it before, and deſired them to examine it a ſecond time; which they did, on the 15th September, and were immediately convinced, that the os femoris was diſlocated, and that it was the head of the bone we felt through the glutæi muſcles, in the ſpace between the iſchium and ſacrum.

To

To the head of the bone's lying in this ſpace, it was probably owing that we were at firſt deceived; ſince its being there allowed a greater latitude of motion, than could poſſibly have been the caſe any where elſe; which may ſerve to account for the paralleliſm of the limb to that of the other, notwithſtanding a luxation.

But, on turning the foot inward and outward by turns, while an aſſiſtant laid his hand on the head of the bone, a grating was perceived, both by the patient and us all. This ſomewhat ſurprized us at firſt; but, as this grating was never to be perceived, without preſſing pretty hardly on the head of the bone, at the time the limb was turned round, and as the head of the bone was plainly felt to turn round, whenever the thigh had that motion given to it, we concluded it could be nothing but the ſide of the head of the femur againſt the edge of the iſchium.

Convinced of this, we determined to make an extenſion; and, to that end, brought him to the foot of the bed, and placed him on his back, with the bed's poſt between his thighs, which was wrapped round with cloths, to prevent its galling him. A napkin was tied round his leg, at the ancle, which two aſſiſtants pulled on, while a third turned the knee outward, and I had my hand on the head of the bone, preſſing it downward. As ſoon as the extenſion began, I perceived plainly the bone ſink under my hand, which I had hardly time to ſay, before it gave a ſnap, which was felt by the patient, and heard by us all, and the bone was reduced.

When he was laid on his back in the bed, the right leg appeared ſomewhat longer than the other; but

but that might poſſibly be owing to the muſcles not immediately overcoming the effect of the extenſion ſufficiently, to draw the head of the femur to the bottom of the acetabulum.

In about ſix or ſeven days he was eaſy, and able to walk over the room with crutches, and bear a conſiderable weight of his body on the right leg; and, from this time, he recovered ſtrength very faſt, and is now, and has long been, as ſtrong in that leg and thigh, as in the other, without any even the leaſt difference in length, or any other reſpect.

To Dr. Huxham.

SIR, Plymouth, May 1760.

I Have ſent you the above caſe of John Down; becauſe I know it is aſſerted by ſome ſurgeons, and among thoſe of the greateſt character too, that a luxation of the head of the femur is little leſs than impoſſible; and that what is generally taken for a luxation of this joint, is a fracture of the bone at its neck.

A fracture of the neck of the bone probably happens more often than a diſlocation. But the above caſe has convinced me, that it may happen, and that without any extraordinary violence, provided the force is aptly applied. Indeed any force applied in the direction of the thigh downward can hardly have any tendency to diſlocate it at all; and any force from below upward will be ſuſtained by the head of the femur bearing againſt the upper part of the acetabulum, till the neck, the weakeſt part, gives way.

But though it may not be poſſible, that the femur ſhould be luxated by any force applied in a direction parallel to that of the thigh, in an erect poſture of the body, it is not equally impoſſible it ſhould be diſlocated by a force applied in a contrary direction. For, in the above caſe, the blow was received on the upper and hinder part of the thigh, in a direction forward, from the wheel of the chaiſe, which muſt neceſſarily have a tendency to drive it round forward, and conſequently cant the head of the femur out of the acetabulum, backwards, where it is leſs deep than it is either above or below. On the fore part of the acetabulum, it is yet more ſhallow, and therefore leſs force is required to diſlocate it that way, and more eſpecially, as there is on that ſide leſs ſtrength of muſcles to reſiſt.

It was probably a fortunate circumſtance for this man, that I thought myſelf miſtaken in my firſt opinion of its being a diſlocation; for, had that been clearly the caſe, I ſhould, no doubt, have uſed every method, and every aſſiſtance to be had, in order to have reduced it immediately; and moſt likely, while the muſcles remained in their full ſtrength, and contracting involuntarily, and that violently too, as they will ſometimes do, upon attempting an extenſion of them, and under which circumſtance, the muſcular fibres oftentimes rather break than give way, ſhould have failed of being able to reduce it; and, in that caſe, the man muſt have remained a cripple as long as he lived: whereas now, though five-and-twenty days from the time of the accident, the muſcles were ſo much weakened, by his being confined to his bed, and waſted by his frequently repeated purges,

that

that they very eaſily gave way, and the reduction was effected with as little difficulty, as ever I ſaw in a diſlocation of the humerus.

Might not the giving ſtrong purgatives, and frequently repeating them, ſo as to render the muſcles of ſtrong muſcular ſubjects more lax and weak, be a means of reducing luxations of the humerus, which are not reducible by any method of extenſion, which is often found the caſe?

I am, SIR,

Your very humble ſervant,

Charles Yonge.

LXXVI. *Some Obſervations upon a* Samnite-Etruſcan *Coin, never before fully explained. In a Letter from the Rev.* John Swinton, *B. D. of* Chriſt-Church, Oxon. *F. R. S. to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society.*

Reverend Sir,

Read June 19, 1760. I Have in my ſmall collection a ſilver Etruſcan coin, of the ſize of the conſular denarii, ſimilar to one publiſhed by (1) Fulvius Urſinus, (2) Patin, (3) Vaillant, (4) Havercamp,

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(1) Fulv. Urſin. in *Famil. Romanar. num. incert.*
(2) Car. Pat. ibid.
(3) J. Vaill. in *Famil. Romanar. num. incert.*
(4) Sig. Haverc. apud Andr. Morel. in *Theſaur. Numiſmat.* p. 459.

and Sig. Annibale degli Abati Olivieri (5), a gentleman of Peſaro, who has written two learned diſſertations upon three or four medals of the ſame kind; all which, with the greateſt appearance of truth, he takes to have been ſtruck on occaſion of the Social war. Fulvius Urſinus, Patin, Vaillant, Morel, and Havercamp aſſign the medal reſembling mine a place amongſt the uncertain, or unknown, conſular coins; though Havercamp (6), without any manner of foundation, affirms it to be a medal of Faleria. But this is not to be wondered at, as he ſeems to have been almoſt an intire ſtranger to the Etruſcan character. Sig. Annibale degli Abati Olivieri has, with the higheſt degree of probability, determined it to be (7) a Samnite coin; and clearly evinced it to have firſt appeared in (8), or about, the year of Rome 663.

But notwithſtanding this moſt learned and ingenious gentleman has ſo perfectly well explained three very rare ſilver Etruſcan medals of C. Papius Mutilus, who commanded the Samnite troops in the Social war, that not the leaſt doubt can be entertained of the truth of his explication; notwithſtanding he has ſufficiently proved the coin I am conſidering to have been adorned with the name of the pretor of the

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(5) *Saggi di Diſſertazioni Accademiche pubblicamente lette nella Nobile Accademia Etruſca dell' antichiſſima Città di Cortona.* Tom. II. p. 49—72. & Tom. IV. p. 133—148.

(6) Sig. Haverc. apud Andr. Morel. in *Theſaur. Numiſmat.* ſive Sig. Haverc. *Comment. in Famil. Roman. num. omn. &c.* p. 459.

(7) Annib. degli Abati Olivier. in *Saggi* &c. ubi ſup.

(8) Id. ibid.

Marſi,

Marſi, who likewiſe diſtinguiſhed himſelf againſt the Romans, in the ſame ruinous and deſtructive war; yet I cannot prevail upon myſelf abſolutely to congratulate him, upon his diſcovery of all the Etruſcan characters forming that name. His diſſertation here does not come quite up to the point. Of this he ſeems himſelf to have been aware, when he informed the learned world, that, " after having viewed the " Etruſcan inſcription with a very good glaſs, and " well conſidered it, he thought he could make out " the following letters, P. ITEEIV; and that if ſuch " another medal ſhould hereafter be found, with the " elements better preſerved, it might perhaps con- " firm his (9) conjecture." Having therefore a ſimilar coin now in my poſſeſſion, I hope I ſhall be indulged the liberty of tranſmitting to the Royal Society a few brief and conciſe obſervations upon it; eſpecially, as theſe may perhaps throw ſome new light upon Sig. Annibale degli Abati Olivieri's diſcoveries, and ſerve as a ſhort ſupplement to his two moſt learned and elaborate diſſertations.

The laureated head on my Etruſcan denarius is adorned with an ear-ring, not viſible either on Sig. Annibale degli Abati Olivieri's medal, or any of thoſe publiſhed by Fulvius Urſinus, Dr. Patin, Dr. Vaillant, and Mr. Havercamp. There is a minute difference between the lower part of the lance, held in the right hand of the galeated figure on the reverſe of the piece before me, and the ſame part of the lance, exhibited by the other coins. The drapery likewiſe of the galeated figure on the firſt of theſe is

(9) *Sag. di Diſſertaz. Accademich.* &c. Tom. II. p. 65, 66.

ſome-

ſomething different from that of the ſame figure on the reſt. The foot of the ox or bull ſeems hairy on my denarius, but not ſo on the others. All which ſingularities, together with the additional Etruſcan element at the end of the inſcription, repreſenting R, and the Etruſcan M on the reverſe, will perhaps intitle the medal I am conſidering to the denomination of an inedited coin. The workmanſhip is good, but ſomewhat different from that of the conſular denarii, particularly thoſe of the ſame age. Mr. Havercamp juſtly remarks, that the legend, or inſcription, here is Etruſcan (10); but by no means aſcertains the forms and powers of the letters, of which it is compoſed. He even took the Etruſcan A in the exergue of his own medal, which does not appear in that of mine, for an (11) inverted R. Sig. Annibale degli Abati Olivieri calls theſe ſmall ſilver pieces (12) Etruſcan medals, becauſe they have ſcarce any thing in common with thoſe of Rome, and are adorned with Etruſcan characters, or ſuch as were in the earlier ages the letters of all Italy, then ſubject to the Etruſcans; but at the ſame time he obſerves, that they might with greater propriety, notwithſtanding the authority of the Marquis Scipio Maffei, be termed Italian coins.

As I remember not to have ſeen an alphabet of the Samnite-Etruſcan characters, exhibited to the public; I have taken the liberty to tranſmit one to the Royal Society, deduced from ſome very valuable Samnite-

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(10) Sig. Haverc. ubi ſup. p. 459.
(11) Id. ibid.
(12) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 49.

Etruſcan

I.



Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

II.



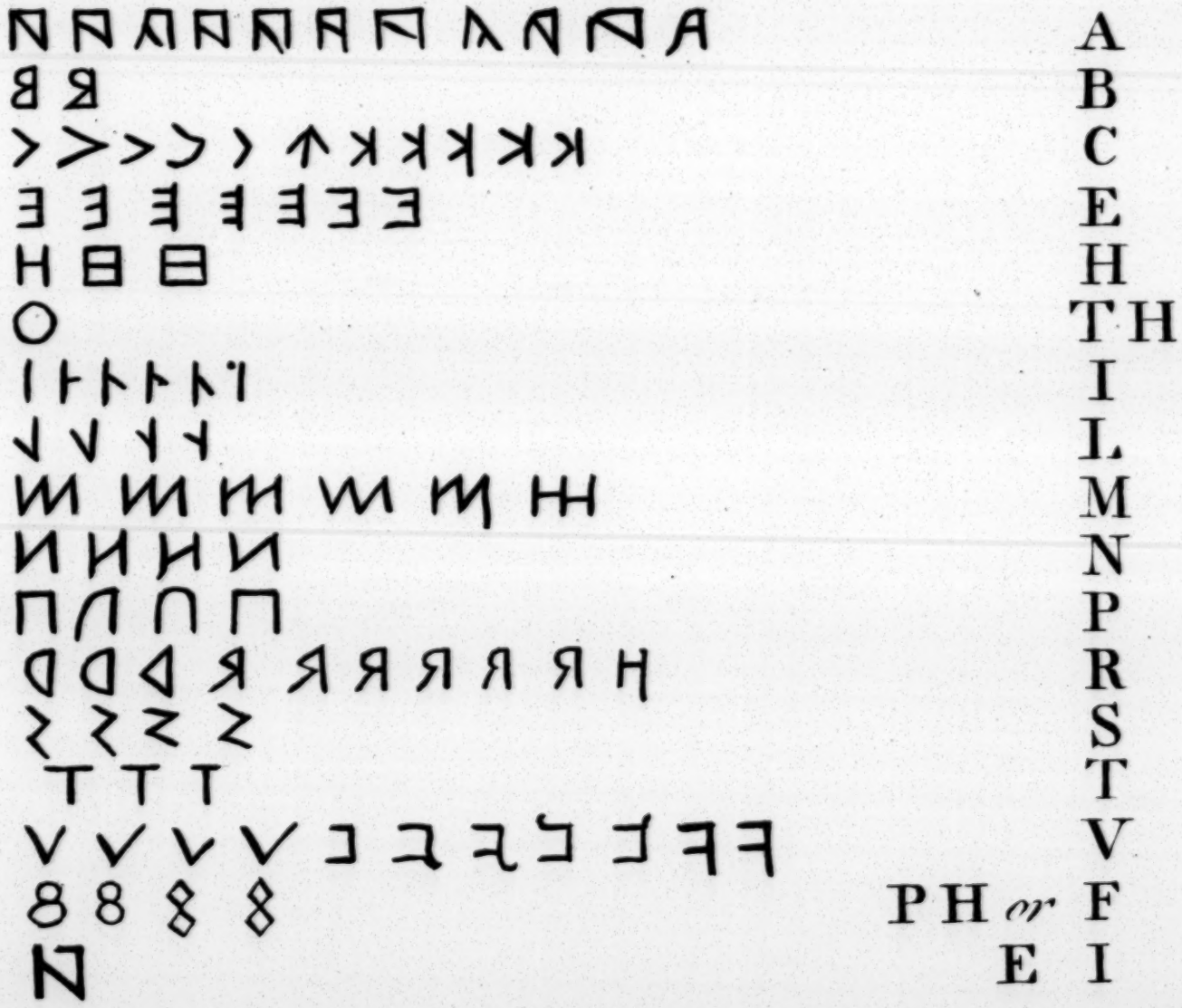
Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

III.



In Gaz. Pemb.

The Samnite-Etruſcan *Alphabet, deduced from the Coins of* C. Papius Mutilus, Veturius, Capua, Acerræ, &c. *and uſed in ſeveral parts of Italy ſo late as the Year of Rome* 663.



J. Hynde ſc.

Etruſcan coins. The ducts of the letters are drawn with ſo much accuracy, that they may be intirely depended upon. [*Vide Tab.* XX.]

The inſcription here mentioned, ſtanding behind the laureated head on this denarius, is drawn from the right hand to the left, after the Etruſcan manner, and conſiſts of eight letters. The firſt of theſe is the *Digamma*, equivalent to V or F, as it appears on the celebrated tables of Gubbio, and ſeveral Etruſcan coins. The ſecond is apparently I, and the ſixth will undoubtedly be taken for the ſame element. The third is manifeſtly T, both on my medal and every ſimilar one hitherto publiſhed. The fourth and fifth muſt be E, though the latter of theſe has not been perfectly well preſerved on my coin. The ſeventh will be univerſally conſidered as V, though only part of it is viſible on the piece before me. The eighth is the Etruſcan R, as preſented to our view by one of the medals of C. Papius Mutilus, by the tables of Gubbio, and by other Etruſcan remains of antiquity. It is very perfect and intire, and not exhibited by any ſuch piece that has hitherto appeared (13). Sig. Annibale degli Abati Olivieri ſeems inclined to take the firſt element of the inſcription (14) for P. But this is probably done in order to accommodate the name to the corrupted text of Velleius. The form he himſelf has given us of this letter bears ſcarce any reſemblance to that of the Etruſcan P, on the medals of C. Papius Mutilus; but approaches near enough to that of the

(13) *Tab. Eugubin.* paſſ. Vid. etiam Gor. *Muſ. Etruſc.* Vol. II. & Dempſt. *De Etrur. Regal.* Tom. I.

(14) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 65.

Digamma,

Digamma, or Etruſcan V, above-mentioned. (15) Mr. Havercamp's denarius, as well as mine, and that deſcribed by M. Morel, ſets the point here inſiſted upon beyond diſpute.

From the foregoing obſervations we may collect, that our inſcription, in Roman, or modern Latin, letters, will be FITEEIVR, or VITEEIVR; which, in the cacography of the Etruſcans, as their rude and uncouth manner of writing is termed by Sig. Paſſeri (16), ſeems to anſwer to the VETVRI, or rather VETVRIVS, of the Romans. If this be admitted, it will be allowed extremely probable, that the name of the general, who commanded the Marſi in the Italian war, was FITEEIVR, VITEEIVR, or VETVRIVS, and neither VETTIVS nor INSTEIVS, as Sig. Annibale degli Abati Olivieri ſeems inclinable (17) to believe. This general is called Vettius, or C. Vettius, by (18) Claudius Quadrigarius; Inſteius, or Inſteius Cato, by (19) Velleius; Οὐέτιος Κάτων, Vetius Cato, by (20) Appian; P. Vettius, or P. Vettius Scato, by (21) Cicero; Vellius, or Vellius Cato, by (22) Florus; and T. Vettius, T. Vetius, T. Vetitius, and T. Vietius, in different manuſcripts

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(15) Sig. Haverc. ubi ſup. p. 459.

(16) Jo. Bapt. Paſſer. Piſaurenſ. *De Num. Etruſc. Pæſtanor. Diſſertat.* p. 18. in *Symbol. Literar. opuſc. var.* &c. Vol. II. Florentiæ, 1748.

(17) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 65, 66.

(18) Claud. Quadrigar. apud Senec. *De Benef.* Lib. iii. c. 23.

(19) Vell. Paterc. Lib. ii. c. 16.

(20) Appian. Alexandrin. p. 375.

(21) Cic. *Philip.* xii. 11.

(22) Flor. apud Sig. Havercamp. in not. ad Eutrop. p. 206. Lugd. Batavor. 1729.

and

and printed editions of (23) Eutropius. From whence we may infer, that the name of this general has been corrupted in moſt, if not all, of the texts of thoſe authors; even upon the ſuppoſition, that our coin bears not any relation to him. But as (24) Sig. Annibale degli Abati Olivieri has clearly evinced the truth of the contrary ſuppoſition, the variety of corruptions here produced will add no ſmall weight to what is more than ſuggeſted by the denarius I am conſidering; ſo that the inſcription exhibited by this ſmall medal is even ſupported by thoſe corruptions, and will undoubtedly, in the opinion of the moſt learned men, aſcertain the name of the general, who commanded the Marſi in the Italian war.

In farther eviction of what is here advanced, it may be obſerved, that the proper name VETVRIVS itſelf has alſo been indiſputably corrupted in other paſſages of antient writers. In one of Cicero (25) it has been transformed into Betucius, and in one of Plutarch (26) into Βετέτιος, or Butetius. Which being maturely weighed, it will not appear ſtrange, that this general's family name ſhould have met with ſo hard a fate. For that the piece I am endeavouring to explain is one of his coins, will be acknowleged more than probable, if we conſider with proper attention what has been urged by Sig. Annibale degli Abati (27) Olivieri in favour of

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(23) Eutrop. *Hiſt. Rom.* Lib. v. c. 3. p. 205, 206. Ed. Havercamp. Lugd. Bat. 1729.
(24) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 65, 66.
(25) Cic. in *Bruto.*
(26) Plutarch. in *Quæſt. Roman.*
(27) *Sag. di Diſſertaz. Accademich.* Tom. II. p. 65, 66.

ſuch a notion. He has demonſtrated this denarius and one of C. Papius Mutilus to be of nearly the ſame ſize and workmanſhip, to be adorned with inſcriptions conſiſting of the ſame ſort of characters, and to have the very ſame type on the reverſe. He has alſo inconteſtably proved, from the beſt authorities that can be produced, that C. Papius Mutilus, mentioned by the coins he has ſo learnedly explained, was cotemporary with our Veturius, or, as he denominates him, P. Iteeius, and the general under whoſe conduct the Samnites ſo greatly diſtinguiſhed themſelves, in the aforeſaid ruinous war, againſt the Romans.

But this will ſtill receive a farther acceſſion of ſtrength from another conſideration, relative to the point I am now upon, which ought by no means to be omitted here. Two (28) medals of the Veturian family and one of (29) C. Papius Mutilus exhibit almoſt exactly the ſame type on the reverſe, though the inſcriptions they bear conſiſt of different characters. This amounts at leaſt to a pretty ſtrong preſumption, that Ti. Veturius, whoſe name occurs on one of the Roman denarii, was either the pretor of the Marſi himſelf or related to ſome commander of the allies of the ſame name; as (30) Mr. Havercamp takes the piece to have been ſtruck not long after the Italians had the right of ſuffrage conferred upon them by the Romans, in order to perpetuate the memory of that remarkable event. I ſay, " or " related to ſome commander of the allies of the ſame

(28) J. Vaill. in *Num. Fam. Vetur.* p. 533.
(29) *Sag. di Diſſertaz. Accademich. &c.* ubi ſup.
(30) Sig. Haverc. *Comment. in Famil. Roman. num. omn. &c.* p. 438, 439, 440.

" name;"

" name;" becauſe it cannot perhaps be ſo eaſily proved, that he was our Veturius himſelf. For we are told by Claudius (31) Quadrigarius, in a fragment of his preſerved to us by Seneca, that one of this general's ſlaves killed him, in order to prevent his falling into the hands of the Romans; which ſeems to intimate, that he was dead before the concluſion of the Social war, or at leaſt before the Italians were put upon an equal foot with the Romans. However, his name might have been impreſſed upon this denarius, after his death. Whether or no the Veturian family itſelf deduced its origin from the Marſi, notwithſtanding the ſilence of Fulvius Urſinus, Dr. Vaillant, and Mr. Havercamp, on that head, I ſhall not take upon me to determine.

Be this, however, as it will, from the family name ꟻITEEIVR, FITEEIVR, or VITEEIVR, on the ſilver Etruſcan, or Samnite, medal, which at preſent is the principal object of my attention, we may infer, that the Etruſcan orthography was very rough and unpoliſhed, at leaſt in ſome of the Italian provinces, ſo late as the 662d, or 663d, year of Rome; when that of the Romans, now called the Latin, was arrived at almoſt the laſt degree of perfection. It muſt nevertheleſs be owned, that the later Latin, or Roman, letters (ſo denominated in contradiſtinction to the antient Latin characters, undoubtedly the ſame with the Etruſcan, and uſed by the Romans themſelves in the earlieſt times) as well as the Etruſcan, were in vogue amongſt ſeveral of the Italian ſtates, when the medals of C. Papius Mutilus and Veturius were ſtruck. This moſt evidently appears, from the ſilver

(31) Claud. Quadrigar. apud Senec. ubi ſup.

 pieces,

pieces, with the word ITALIA upon them, explained by Sig. (32) Annibale degli Abati Olivieri, in his firſt diſſertation; and from a ſimilar one in my little cabinet, hitherto unpubliſhed, which I lately acquired. Hence ſome learned men may perhaps be inclined to believe, that both the denarius of the Veturian family and the coin by me here referred to our Veturius did in reality exhibit the name of the general of the Marſi, and firſt appeared about the 663d year of Rome. With regard to the word ⅁VIƎƎTIↃ, or VETVRIVS, mentioned in the beginning of this paragraph, I ſhall beg leave to remark, that it might as eaſily have been transformed into VETTIVS, or VETIVS, in the manuſcripts and printed editions of ſome antient writers, as PAPIVS into APONIVS in thoſe of others; and that the (33) word APONIVS, for PAPIVS, ſtands in the text of Diodorus Siculus, as cited by Photius. With which obſervations I ſhall conclude what I have here to ſay, in defence of my lection of this Etruſcan inſcription.

As I only intend this paper to be a ſhort ſupplement to Sig. Annibale degli Abati Olivieri's two moſt excellent diſſertations upon his Samnite coins, it would be intirely ſuperfluous to attempt an explication of the types exhibited by thoſe coins; eſpecially, as this has already been done in ſo ample a manner, with ſo much ſagacity and erudition, by that very ingenious gentleman himſelf, the learned Sig. Bianconi, and the moſt acute Sig. Paſſeri, that ſcarce any thing new or material on that head can be added to their obſervations. However, it may not be improper

(32) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 57.

(33) Diod. Sic. apud Photium, in *Myriobibl.* p. 1185, 1186. Ed. Schot. Rothomagi, 1653.

juſt

juſt to remark, that both the denarii of C. Papius Mutilus and the piece by me attributed to our Veturius (34) were coined, in commemoration of the alliance entered into by the Italian ſtates againſt the Romans, about the time of the commencement of the Social war; whereas the Roman medal of Ti. Veturius (35), according to Mr. Havercamp, was probably ſtruck after the concluſion of that war, and even after the admiſſion of the Italian allies into the city, the memory of which it was perhaps intended to perpetuate, by the Romans.

That the Etruſcan pieces here conſidered were ſtruck on occaſion of the Italian war, or war of the allies, is likewiſe abundantly clear, from a moſt rare Etruſcan medal in Lord (36) Pembroke's noble collection, never hitherto explained. It is adorned with ſuch a laureated head as occurs on Veturius's coin, and on the reverſe a bull goring with one of his horns a furious wild beaſt, reſembling a monſtrous dog, he had got under him, together with the Etruſcan inſcription, VTNCBNIƎ ИITVIM IΠNNΠ >, C. PAAPI MVTIL EMBRATVR, though very indifferently copied. This is a fourth Etruſcan, or Samnite, coin of C. Papius Mutilus, the Samnite general; which Sig. Annibale degli Abati Olivieri had neither ſeen nor heard of, when he publiſhed thoſe two learned diſſertations, which this paper is intended to render a little more complete. The bull on Lord Pembroke's medal undoubtedly denoted the ſtates of Italy, or rather

(34) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 65—72. & Tom. IV. p. 140—148.

(35) Sig. Havercamp. *Comment. in Famil. Roman. num. omn. &c.* p. 438, 439, 440.

(36) *Numiſm. Antiqu. &c.* Thom. Pembroch. et Mont. Gomeric. Com. P. 2. T. 87. num. 10.

the

the union of thoſe ſtates, combined againſt the Romans, as the fierce animal under him did the ſenate and people of Rome; the whole type pointing out to us the hoped for ſuperiority of the former over the latter, or rather being intended as a prognoſtication of ſuch ſuperiority. That the bull was the ſymbol of ſeveral Italian cities, or ſtates (37), appears from the coins of Poſidonia, Thurium, Arpi, *&c.* and that the (38) word Ἰταλὸς, Italus, or Italian, in the old Etruſcan tongue, the primitive language of all Italy, ſignified a bull, whence probably that animal might become the ſymbol of at leaſt a very conſiderable part of this country, is a point ſo exceeding clear, that it will not admit of a diſpute.

To what has been ſaid we may add, that the Italians looked upon the Roman republic, about the time of the Social war, as a receptacle of wolves, always ready, if not determined, to raviſh their liberty from them. This we may collect from the following words of Pontius Teleſinus, in Velleius: (39)—*adjiciens, nunquam defuturos raptores Italicæ libertatis lupos, niſi ſilva, in quam refugere ſolerent, eſſet exciſa.* This obſervation may poſſibly throw ſome new light upon Lord Pembroke's medal, as well as upon the type of my Etruſcan denarius, and the ſimilar one of C. Papius Mutilus's coin, which ſeems to have a little embarraſſed the learned (40) author I have all along here had principally in view.

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(37) Hubertus Goltzius, in *Magn. Græc. Numiſm.* paſſ.

(38) Apollodor. Lib. ii. Iſacius ad Lycophronem, & Tzetzes in *Chiliadibus.*

(39) Vell. Paterc. Lib. ii. c. 27.

(40) *Sag. di Diſſertaz. Accademich. &c.* Tom. II. p. 66, 67. & Tom. IV. p. 141.

As

As the foregoing Etruſcan coins are proved to have been ſtruck about the 662d or 663d (41) year of Rome, the Etruſcan alphabet then undoubtedly prevailed over a very conſiderable part of Italy; though the forms of ſome of the characters, particularly thoſe of A, P, and I, were not preciſely the ſame with thoſe of the equipollent letters uſed in Umbria and the (42) Proper Etruria. I ſhall only at preſent beg leave farther to remark, that the reverſe of my Etruſcan denarius has handed down to us a new form of the Etruſcan M, placed in the exergue, which has never been before obſerved on any of the Etruſcan remains of antiquity; and that I am, with ſentiments of the higheſt reſpect and eſteem,

SIR,

Your moſt obliged,
and moſt obedient,
humble ſervant,
J. Swinton.

Chriſt-Church, Oxon.
May 19, 1760.

LXXVII. *De Proximo Veneris ſub Sole Tranſitu.*

Ad Londinenſem Regiam Societatem.

Read June 19, 1760.

1. NEMO ſanè ignorat Aſtronomus, quanti interſit ad abſolutam perfectionem Aſtronomiæ, accurate noſſe diſtantiam Terræ

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(41) Annibal. degli Abat. Olivier. ubi ſup. & alib.
(42) Gor. *Muſ. Etruſc.* Annibal. degli Abat. Olivier. in *Sag. di Diſſertaz. Accademich. &c.* Tom. II. & Tom. IV.

à Sole, à qua reliquorum Planetarum, Cometarumque pendent abſolutæ diſtantiæ, moles, ac maſſæ, et alia ſane multa, quæ notam exigunt parallaxim Solis cum hac ipſa diſtantia tam arcte connexam, ut utriuſlibet inveſtigatio pro uno eodemque haberi ſoleat. At hoc tantum nobiliſſimæ facultatis fundamentum nondum intra ſatis arctos limites definitum eſſe cenſetur paſſim vel poſt ipſos, utiliſſimos illos quidem, Caillii labores, ex quibus neſcio, an omnino certum intra decimam ſui partem habendum ſit. Quamobrem nulla videtur prætermittenda occaſio promovendæ ulterius nobiliſſimæ atque utiliſſimæ perquiſitionis, quod ad eos maximè pertinet doctorum hominum cœtus, qui publica auctoritate et conſtituti et ſuſtentati multo facilius, quam privati homines et inire poſſunt, et vero etiam debent, rationes ad rem perficiendam idoneas.

2. Una ex occaſionibus rei bene gerendæ maximè opportunis habebitur ſequenti anno, nimirum expectatiſſimus et rariſſimus Veneris ſub Sole tranſitus; quæ occaſio ne ſine optatiſſimo fructu effugiat, id vero potiſſimum ad Anglicanam nationem curare pertinet, et ad hanc Regiam Societatem in primis. Duplex autem videtur mihi eſſe ratio, cur hujus occaſionis non omittendæ cura ad Angliam in primis, et ad hunc doctiſſimum cœtum, pertineat. Primo quidem, quod in ejus occaſionis ſpem jam ab anno 1716, in Philoſophicis Tranſactionibus univerſam primus Europam erexit ingens illud Aſtronomiæ lumen, ac Anglicanæ nationis decus, Halleyus, Aſtronomos omnes ad eam rem excitans, quem et illud maxime doluiſſe conſtat, quod nullam haberet ſpem vitæ uſque ad id tempus producendæ, ut tanto bono frui poſſet. Deinde, quod nuſquam alibi inſtrumenta ad rem bene

gerendam

gerendam apta ad eum perfectionis gradum, quo hic eſt opus, redacta ſint, ut hic in Anglia.

3. Primum caput per ſeſe patet; quam enim dedeceret illud, ſi, quod ne ſuſpiciari quidem poſſum, ea gens tantam opportunitatem negligeret, quæ ipſum detexit prima, quæ cæteras nationes ad eam arripendam incitavit, quæ tot alia præclariſſima inventa ad Aſtronomiam in primis perficiendam huc uſque protulit! Ipſi nimirum debet Literaria Reſp. generalem immortalis Newtoni gravitatem, quæ una ad Cœlum Aſtronomis reſerandum cenſetur jam per univerſam Europam quædam veluti clavis certiſſima; ipſi Cometarum orbitas, et Lunarium aberrationum periodum ab Halleyo definitas; ipſi aberrationem luminis, et nutationem axis incredibili et patientia, et ſagacitate, et vero etiam felicitate detectam à Bradleyo, à quo uno, definitis jam demum ſtellarum fixarum locis ad datum tempus quodcumque intra arctiſſimos unius, vel alterius ſecundi limites, Aſtronomia accepit illud, ut perfici demum poſſit; ipſi alia innumera, quæ tantam Anglicanæ nationi laudem et gloriam pepererunt, quibus omnino vel tantummodo commemorandis nullus adeſſet finis. Quamobrem huc univerſa defixos habet oculos Europa, nihil dubitans, quin eadem perſtet adhuc ad nobiliſſimam facultatem promovendam alacritas, ac vetuſtum omnium bonarum artium ſtudium perſeveret, et qui alios ad hanc ipſam rem bene gerendam incitarunt ſcriptis, exemplo quoque ſuo ſollicituri ſint.

4. Quod autem ad alterum pertinet caput, inſtrumenta omnino neceſſaria ad hujuſmodi obſervationem ſunt horologium oſcillatorium et teleſcopiam: horum alterum hic primum ſatis perpolitis, et rite diſpoſitis

tam variis delicatiſſimæ machinæ partibus, ac impedito caloris effectu, ſummam demum æquabilitatem accepit, nec uſquam alibi nunc etiam majorem habet perfectionem, quam hic: alterum hinc fere tantummodo prodit ſatis idoneum ad ſubtiliores ejuſmodi obſervationes, ſive dioptricum adhibendum ſit, ſive catadioptricum. Illud quidem poſt mirum ſane recentiſſimum Dollonii inventum ad eum perfectionis apicem redactum eſt, quem olim nunquam ſperare licuiſſet: hoc autem hic primo inventum, eo Shortii in primis diligentia atque induſtria perduxit, ut hinc potiſſimum ejuſmodi inſtrumenta præcipui omnes per Europam Aſtronomi ſibi expetant, atque hic, quotieſcumque per ſumptus licet, requirant. Sed et reliqua illa, quæ uſui eſſe poſſunt ad uberiores certioreſque ex hoc Veneris tranſitu capiendos fructus hic vel inventa ſunt primo, vel ad ſummum perfectionis gradum perducta, quadrantes nimirum, ſectoreſque, et micrometra, tam externa illa, quæ ad alidadæ motum pertinet, quam interna, ſive ea ſint filaria illa vetera, quæ ocularia appellantur, ſive hæc nova, quæ objectiva dicimus, quorum abſolutam perfectionem debemus itidem Dollonio, in quibus omnibus, ut omittam cæteros, tantam ſibi laudem Siſſonius comparavit. Ea ſane omnia nuſquam alibi ita ad rem idonea conſtruuntur, ut hic. Quamobrem quis illud plane non agnoſcat, eam gentem, quæ inſtrumenta ad utiliſſimas obſervationes inſtituendas præ ceteris omnibus aptiſſima exhibeat, decere itidem ante omnes alias iiſdem uti, et Aſtronomos, quos habet plurimos, ad capiendos ex iiſdem fructus ab omni Europa expectatos adhibere.

5. Accedit

5. Accedit æmulæ nationis conatus. Nam ante meum diſceſſum Pariſiis profectus jam fuerat unus ex Academicis Pondicherium in India, alter à Petropolitana requiſitus Academia parabat ſe ad diſcedendum Tobolſkam in Siberiam: eodem autem, quo diſceſſi, die, de tertio ad caput Bonæ Spei mittendo deliberabatur, eratque alius, qui ſe offerret ad navigandum in Cyprum inſulam, ac rem urgebat. Uſque adeo ne in nobiliſſima facultate promovenda vinci ſe ſinet à Gallis Anglia, quæ tanto majorem hoc potiſſimum tempore opportunitatem habet tranſvehendorum quocumque oporteat Aſtronomorum?

6. Hæc quidem ad Angliam privatim pertinent, illud vero ad gentes omnes, phænomeni raritas ſumma, quæ hanc vetat occaſionem omittere. Id quidem phænomenum huc uſque à nullo unquam Aſtronomo eſt obſervatum. Nam Horoccius partem vidit tantummodo Sole ante finem demerſo, nec alios longe poſitos obſervatores habuit, cum quibus illam ipſam ſuæ obſervationis partem conferret. Idem phænomenum non niſi poſt annos octo recurret, tum nunquam iterum per totum ſæculum, eoque amplius, habebitur. Quo pauciores ſeſe occaſiones offerunt, eo ipſas avidius arripiendas eſſe nemo non videt. Sed hiſce, quæ ad incitamenta pertinent, omiſſis, ea, quæ ſpectant ad ipſam obſervandi rationem, et loca obſervationibus inſtituendis maxime idonea, paucis evolvam.

7. Omnis obſervatio in eo potiſſimum eſt ſita, ut accurate determinetur momentum, quo nigricans Veneris diſcus lucidum Solis diſcum contingit. Horum contactuum habentur quatuor, bini in immerſione, bini in emerſione. Initium immerſionis et finis emerſionis

non ita accurate definiri poſſunt, cum in illo incipiat, in hoc deſinat excavari in Solis limbo foveola quædam, quæ, ubi ſatis exigua eſt, facile oculos vel maxime diligentes fallet. Nam centrum Veneris horis circiter 6 percurret in Sole chordam minutorum circiter 25 diſtantem à centro Solis minutis circiter $9\frac{1}{2}$, exiſtente ſemidiametro apparenti Solis minutorum proxime 16. Calculo inſtituto, invenio Venerem debere impendere ſecunda horaria $14\frac{1}{2}$ ad percurrendum in Sole unum ſecundum in directione ejus chordæ, et in ipſo ingreſſu vel egreſſu ob ejuſdem chordæ obliquitatem, erit tardior in ratione chordæ ad diametrum Solis, adeoque acceſſus per unum ſecundum requiret aliquanto etiam plus quam 18 ſecunda temporis; cumque foveola, quæ uno tantummodo ſecundo intra diſcum Solis procurrat, ſub ſenſum cadere aut vix, aut ne vix quidem, poſſit, patet ne intra quartam quidem minuti partem ſperari poſſe hujus initii et finis determinationem ex obſervatione immediata. Licebit illud initium quærere ex chordis ſegmenti obſcuri determinatis per micrometrum objectivum, ubi jam ipſa foveola aliquanto major evaſerit; earum enim ſingulæ, cum ſuam ſagittam determinent, deficiunt ſimul partem diſci Veneris immerſam, adeoque immerſionis initium: adhuc tamen ſatis vereor, ne ea etiam methodo citra plurium ſecundorum errorem ſatis definiri non poſſit illud primum immerſionis initium, et ille ultimus finis emerſionis.

8. At finis immerſionis et emerſionis initium determinari omnino poſſunt cum evidentia ſumma, et perquam exactiſſimè. Dum enim Venus in immerſione progreditur, et jam proxima eſt immerſioni totali, habentur ut in figura I.

FIG.

FIG. I.

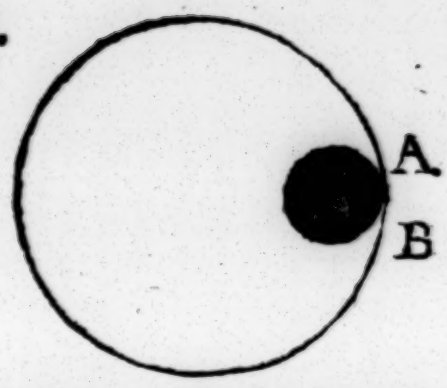
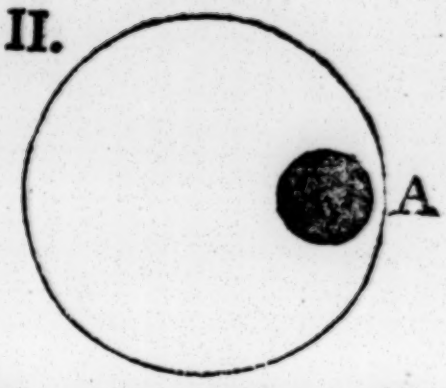


FIG. II.



bina in Sole veluti cornua A, et B, quæ minimo Veneris motu in fine coeunt, abeunte figura prima in ſecundam, in qua quoddam luminis veluti filum in A ingricantem Veneris rotundum diſcum cingit: contra in emerſione abrumpitur citiſſime filum illud, et ſecunda figura abeunte in primam, bina illa veluti cornua enaſcuntur. Momentum, quo ipſa cornua coeunt, vel quo enaſcuntur, ob ipſam celeritatem, qua binæ cuſpides ad ſe invicem vel ultimo accedunt, vel primo recedunt, definiri poterit admodum accuratè.

9. Res facile patebit calculo inſtituto, qui eſt admodum elementaris. Erit enim diameter Veneris proxime unius minuti primi, et diſtantia cornuum, quæ eſt chorda communis diſci Solis, et Veneris, erit in initio et fine media proportionalis inter partem diametri Veneris extantem extra Solis diſcum, et quadruplum reſiduæ partis ejuſdem diametri quamproximè, neglecta nimirum curvatura Solaris diſci tanto majoris reſpectu curvaturæ diſci Veneris, cujus Solaris curvaturæ ſi habeatur ratio, adhuc major evadit chorda. Inde autem eruitur illud, uno ſecundo horario, vel binis, tribus, quatuor ante finem immerſionis, vel poſt emerſionis initium diſtantias cornuum A, B à ſe invicem fore proximè ſecundorum $3\frac{1}{2}$, 5, 6, 7, creſcentes nimirum in ratione temporis ſubdupli-

ſubduplicata. Porro vel ipſa prima ex hiſce quatuor diſtantiis ſecundorum $3\frac{1}{2}$ ſelectioribus teleſcopiis adhibitis omnino ſub ſenſum cadit, ſecunda ſecundorum quinque etiam communibus non prorſus ad rem ineptis, adeoque optimis teleſcopiis intra unum etiam ſecundum, communibus intra duo, ea phaſis poterit determinari.

10. Quod ſi conſideretur craſſitudo fili lucidi in A in fig. II. ea ſingulis ſecundis horariis augetur per $\frac{1}{18}$ partem unius ſecundi, quæ ipſa in tam vivido Solari lumine utut repreſſo à coloratis vitris, vel ſaltem ejus duplum, ſatis manifeſto deprehendi debet.

11. Videtur igitur illud omnino ſperari poſſe, ut hæc temporis momenta intra unum etiam, vel alterum ſecundum, abſolute determinentur. Verum in re noſtra nec id ipſum neceſſarium eſt niſi ad elementa Veneris accuratius determinanda. Ut eruatur id, quod pertinet ad parallaxim, et diſtantiam Solis conſideranda eſt ſola differentia errorum, quos bini obſervatores committent ex radiis aberrantibus, qui inducunt procurſum cornuum, et eorum diſtantiam minuunt, vel ex tenuitate fili lucidi non ſatis percellente oculi aciem. At hæc differentia et multo minor eſſe debet errore integro, ut patet, et in immenſum imminui poteſt, ſi bini obſervatores teleſcopia adhibeant ejuſdem vis diligenter inter ſe invicem ante collata. Ego ſane nullus dubito, quin ſi hæc adhibeatur induſtria, poſſit errorum differentia detrudi vel infra unium ſecundum temporis, quam etiam ſine illa omnino arbitror, modo teleſcopia ſint ſatis bona, ultra duo ſecunda non procurſuram.

12. Ejus rei argumentum præter ea, quæ huc uſque diximus, eſt mihi et illud, quod eundem

Mercurii tranſitum Pariſiis obſervantes ſimul tres vel quatuor Aſtronomi, conſenſerint in ejuſmodi phaſi obſervanda intra quinque ſecunda, uti in poſtremo ex Pariſienſis Academiæ conſeſſibus, quibus interfui, enunciatum fuit ſub ipſum meum diſceſſum. Porro primo quidem in eo tranſitu Mercurius aliquanto minorem potius habuit celeritatem in Sole, quam Venus ſequenti anno habitura ſit; deinde cum diameter apparens Mercurii circiter ſextuplo minor, quam Veneris diameter, apparens ſit futura, diſceſſus cornuam à ſe invicem debuit eſſe ex eo capite minus celer in ratione 1 ad $2\frac{1}{2}$, quam in Venere futurus ſit. Quamobrem hic multo minus diſcrimen timeri poteſt, quam ibi: ibi autem nec illud quinque ſecundorum diſcrimen totum huic cauſæ tribuendum eſt, ſed partim etiam errorculo fortaſſe alicui in horologiis dirigendis, partim diſtantiæ locorum, in quibus eſt obſervatum: unde conſtat, jure nos hic metum diſcriminis ex teleſcopiorum errore orti deprimere infra duo ſecunda horaria; licet nonnulli et decem, et viginti ſecundorum errorem timendum cenſeant, ſed, uti abunde demonſtravimus, omnino perperam.

13. Jam vero ut ex ejuſmodi obſervatione optatus capiatur fructus, bina ad rem idonea, et à ſe ſatis remota, ſeligenda ſunt loca, in quibus obſervationes fiant, quæ inter ſe comparentur, quod triplici ratione fieri poteſt. Prima ratio, et omnium trium maxime idonea, eſt ea ut obſervetur utroque in loco et immerſio et emerſio, comparando bina tempora durationum totius phænomeni; ſecunda, ut utrobique obſervetur immerſio; tertia, ut emerſio utrobique obſervetur, comparando in hiſce poſtremis binis methodis horas locorum, in quibus obſervationes ſunt inſtitutæ,

inſtitutæ, ac horam alterius ad alterius horam reducendo.

14. Primam methodum propoſuit jam olim Halleyus, qui quidem calculo inito cenſuit ad rem maxime idonea fore bina loca, quæ ipſe ſelegerat, alterum in Septentrionali America ad Hudſoniam vallem, alterum in Aſia Orientali circa oſtia Gangis, quibus in locis differentiam durationis totius phænomeni cenſuerat fore minutorum primorum 17, quæ cum efficiant pluſquam 1000 ſecunda; vel 10 ſecundorum error è binis obſervationibus coaleſcens in unam ſummam, intra centeſimam ſui partem definiret Solis diſtantiam et parallaxim, quo quidem errore oſtendimus timeri vix poſſe errorem quintuplo minorem.

15. Is quidem Solis parallaxim horizontalem aſſumpſerat aliquanto majorem, quam nunc cenſeatur, quod illam ejus differentiam durationum auxit nonnihil; ſed id ejus methodo parum obeſſet. Aliud quidpiam, quod gravioris momenti eſt, humani paſſum doctiſſimum ſane virum affirmavit Iſleus in Pariſienſis Academiæ conſeſſu, quod paullo poſt etiam typis edidit, quo quidem correcto invenitur illud, ſi cum Gangis oſtio conferatur locus in Septentrionali America maxime idoneus (nam is, quem ipſe Halleyus ibi nominaverat, definitis aliquanto accuratius per hoſce annos elementis theoriæ Veneris, reperitur extra eos limites, intra quos hic ejuſdem Veneris tranſitus apparebit) vix haberi 4 minuta ejus diſcriminis; et ejus quidem erroris ſe fontem etiam inveniſſe affirmavit Iſleus ejuſmodi, cui ſummi quoque viri ſunt aliquando maxime obnoxii, ſubtractionem nimirum binorum angulorum adhibitam pro additione:

tione: angulorum alter eſt inclinatio orbitæ Veneris ad eclipticam graduum 8° 28′, alter circulatio circuli latitudinis ad circulum declinationis 6° 10′, quos, ut Iſleus affirmat, oportuit ſimul addere; ille autem alterum ab altero ſubtraxit.

16. Et mihi quidem ante hoſce tres annos in ipſum diſcrimen inquirenti longe alia methodo, obvenerat itidem diſcrimen ipſum perquam exiguum. Conſiderabam enim illud, duplicem eſſe diſcriminis fontem, alterum ex eo, quod ſi ſeligantur bina loca hinc, et inde à polo diurnæ vertiginis, in altero, nimirum in Septentrionali America, motus diurnus debeat eſſe conformis motui Veneris, in altero ex parte oppoſita ſito contrarius; unde fit, ut cæteris paribus in priore loco phænomenum diutius durare debeat, quam in poſteriore, uti et ſi binæ naves in eandem plagam tendant, diutius in certa parte littoris ad latus ſiti interior exteriori apparebit, breviore vero tempore, ſi ferantur in partes oppoſitas: alterum diſcriminis fontem videbam eſſe viam ipſam, quæ intra Solem apparebit percurſa à Venere. Ea enim alteri loco erit à centro remotior, adeoque brevior; alteri propior, et proinde longior. Ii bini diſcriminis fontes in unum additi Halleyanum mihi tempus ſatis proxime exhibebant. Et quidem ſi borealis eſſet Veneris latitudo, coirent utique ii bini fontes; nam chorda debita locis ultra polum borealem ſitis centro propior appareret, et longior; at cum Auſtralis ea ſit, videbam ſane contrarium evenire, et ſubtractionem binorum terminorum adhibendam, quæ mihi vix tria vel quatuor minuta relinqueret. Re aliquandiu perpenſa, cum aliis tum quidem longè gravioribus curis diſtinerer, metuerem autem, ne mihi potius admodum

diſtracto, quam tanto et Aſtronomo et Geometræ error aliquis ſubrepſiſſet; meditationem omnem abrupi, quam Iſleus per otium perſecutus rem omnem calculo pluribus vicibus repetito, et conſtructione adhibita, evolvit: et quidem eſt ex ejus diſcipulis, qui Pariſiis impreſſerit, eum ipſum à ſe de errore invento admonitum diu obſtitiſſe, et calculorum iterationem præcepiſſe, donec et ipſe rem diligenter exquirens idem demum deprehenderit.

17. Verum Iſleus ipſe non errorem tantummodo invenit, ſed uti diximus, et erroris fontem aperuit, et longe moleſtius, at ſimul etiam longe utilius opus aggreſſus, mappam binis hemiſphæriis conſtantem delineavit, alteri ſimilem, quam pro Mercurii tranſitu delineaverat olim, cujus exemplar, cum brevi diſſertatiuncula impreſſa, huc ad hanc ipſam Regiam Societatem tranſmiſit. In ea diverſis coloribus diſtinxit quatuor totius ſuperficiei terreſtris partes, in quibus vel ingreſſus tantummodo Veneris in Solem apparebit, vel tantum egreſſus, vel uterque, vel neuter: adjecit autem et lineas colore rubro diſtinctas, quarum ſingulæ exprimant loca omnia, quibus vel ingreſſus accidet, vel egreſſus eodem aliquo momento temporis, quod Pariſienſis meridiani horis deſignavit.

18. Is quidem methodum, qua ad ejuſmodi delineationem eſt uſus, nequaquam expoſuerat ante meum diſceſſum Pariſiis, ſed pluribus id ipſum rationibus inſtitui poteſt, nec theoria eſt ſane difficilis, ſed praxis admodum laborioſa, et longa. Dom. De la Lande unam è methodis ad rem idoneis propoſuit in poſtremo ex Academiæ conventibus, quibus interfui; quam dum is in ſchemate demonſtraret, ego alio evocatus ſatis precipere non potui: at rem ipſam

meditatus

meditatus invenio illud, ſingulas ejuſmodi lineas in ſuperficie Terræ, eſſe totidem interſectiones ſuperficiei coni recti cujuſdam, qui facile definitur, cum ipſa Terræ ſuperficie. Tres autem diverſi coni adhiberi poſſunt, prout quærantur loca, in quibus ſimul videri debeat immerſionis initium, vel emerſionis finis, immerſio, vel emerſio centri, immerſionis finis, vel emerſionis initium. In primo caſu angulus lateris coni cum axe eſt ſumma ſemidiametri apparentis Solis viſi è Venere, et Veneris viſæ è Sole, in ſecundo ſola ſemidiameter Solis, in tertio earum ſemidiametrorum differentia; vertex autem omnium eſt in recta jungente centra Solis et Veneris, ſed in ſecundo caſu in ipſo Veneris centro, in primo citra centrum Veneris Solem verſus, in tertio ultra ipſum Veneris centrum ita, ut ejus diſtantia ab ipſo centro Veneris ad diſtantiam à centro Solis ſit in utroque ex hiſce poſtremis caſibus in ratione ſemidiametri veræ Veneris ad ſemidiametrum veram Solis.

19. Ex hoc theoremate, data Veneris poſitione ad datum tempus, accuratiſſime ea omnia definiri poſſunt; ſed ad ejuſmodi mappam delineandam, quæ rei ſummam quandam unico obtutu conſpiciendam exhibeat, ſatis eſt unum ex ejuſmodi conis conſiderare; quin immo ſatis eſt ejus ſuperficiei partem admodum exiguam in ſuperficiem Terræ incurrentem, et parum admodum inclinatam ad diſcum è Sole viſum aſſumere pro plano perpendiculari ipſi diſco, cujus interſectio cum hemiſphærio ſuperficiei terreſtris aſſumptæ pro ſphærica obverſo Soli erit ſemicirculus ipſi diſco perpendicularis, et ſectio cum ipſo diſco linea recta. Circularis enim ſectio ejus coni cum plano diſci Terræ comparata eſt multo major ipſo diſco. Facile invenio

illud: ſi ab angulo ſupra definito lateris coni cum axe auferatur ſemidiameter apparens Solis viſi è Terra, reſiduum fore ſemidiametrum ejus circuli viſam è Sole, quæ proinde erit ad ſemidiametrum Terræ, ut eſt numerus ex illa ſubtractione proveniens ad parallaxim horizontalem Solis, quæ ratio evadet circiter 40 ad 1, ſi ea parallaxis aſſumatur circiter ſecundorum 10.

20. Inde autem fit, ut delineatis in terreſtri diſco meridianorum et parallelorum projectionibus ea ratione, quæ in Lunæ eclipſibus adhiberi ſolet, admodum facile determinentur loca omnia ad ſingulas ejuſmodi lineas pertinentia; et multo facilius in globo res perfici poſſit, invento prius pro ſingulis ſemicirculis et polo eorum, et diſtantia à polo ipſo, quo pacto unico circini ductu res omnis perficitur. At pro locis, in quibus ingreſſus Veneris vel egreſſus habetur, Sole in horizonte ſito, ſatis erit aſſumere circulos Terræ maximos, qui diſcum terminant è Sole viſum momento temporis, quo è centro Terræ loca ingreſſus centri, vel egreſſus videretur, quæ momenta paucis tantummodo minutis differunt ab iis, quibus alia Terræ loca ingreſſum vel egreſſum videre debent, adeoque illi circuli rem ſatis pro hoc conſtructionum genere accuratè determinant. Iidem autem Telluris ſuperficiem in illas quatuor partes dividunt, quas ſupra expoſuimus, quæ nimirum videre poſſunt vel alterum, vel utrumque, vel neutrum ex iis phænomenis. Sed hæc omnia fuſiore geometrica illuſtratione indigerent, ubi et operationum compendia ſanè multa haberi facile poſſunt.

21. Iſleus quidem nec elementa motuum Veneris, quibus eſt uſus, adhuc, quod ego ſciam, uſpiam expoſuit;

expoſuit; ſed illud pro certo habeo, tabulas ab ipſo eſſe adhibitas optimæ notæ, quas adhuc per ſelectas recentiores obſervationes correxerit. Ipſa quidem elementa, et potiſſimum hujuſce nodi poſitio non ita certa ſunt adhuc, ut omnimodus expectari debeat totalis hujus ipſius delineationis conſenſus cum obſervatione ineunda, poſito etiam, quod horizontalis parallaxis Solis ſit ea ipſa, quam Iſleus aſſumpſit, ſed ita parum à vero aberrare poteſt, ut, quod pertinet ad delectum locorum inſtituendis obſervationibus idoneorum, et fructum, qui ex iis ſperari poſſit, quæcumque è tribus ſupra propoſitis comparationum rationibus adhibeatur, tuto illi fidere liceat. Ad parallaxim ipſam accuratè determinandam oportebit ex ipſis obſervationibus hujuſce tranſitus corrigere elementa omnia motuum Veneris circa hunc nodum, et inde accurato calculo inſtituto pro iis ipſis locis, in quibus obſervatum fuerit, inquirere differentiam temporum iis debitam in hypotheſi parallaxeos ſolaris ad calculum ineundum adhibitæ, quæ ad inventam per obſervationes erit quamproximè in ratione reciproca differentiæ temporis calculo erutæ ad differentiam obſervationibus definitam, ſiquidem diſcrimen earum differentiarum exiguum fuerit, uti eſt admodum probabile; ſecus vero ope parallaxeos primo correctæ reſtituto calculo, brevi poterit per ſeriem admodum convergentem ad verum ipſius parallaxeos valorem deveniri.

22. Unum illud ad Iſlei mappam pertinens hic omnino addendum eſt, ejus impreſſionem binis fieri cupris, quorum altero Terræ loca nigro colore exprimantur, altero lineæ ad Veneris tranſitum pertinentes colore rubro. In pluribus ex exemplaribus impreſſis non

non ita diligenter ſecundo cupro idem chartæ folium eſt impoſitum, ut rite omnia congruant, quod quidem ipſi exemplari contigit ad Regiam Societatem tranſmiſſo, in quo idcirco nonnullis eſt viſus ipſius delineationis error graduum 10. At ſigna quædam adſunt in ipſis exemplaribus, per quæ ipſe error deprehendi, et corrigi utcumque poſſit, et in exemplari, quod ego ad Societatis præſidem detuli, ita ambæ impreſſiones congruunt, ut vix ullus ejuſmodi error ſuperſit, nec is generalem rei ideam, quam hic quærimus, quidquam perturbat.

23. Ut vero ex hac ipſa mappa jam inquiramus in loca tribus illis comparationum methodis idonea, illud occurrit in primis admodum facile percipiendum, primam methodum, quæ adhibet durationem totius phænomeni, reliquis binis eſſe multo aptiorem. Nam in ea multo minus accurate neceſſarium eſt noſſe binorum locorum longitudines. Sub eodem Capricorni Tropico à longitudine graduum 75 ad longitudinem 130 ubique totius phænomeni duratio ita evadit horarum 6, ut non niſi aliquo ſecundorum numero inde diſcedat. Quamobrem error ſatis ingens in longitudine locorum determinanda nihil ad ſenſum perquiſitionem perturbat. Secus evenit in reliquis binis, in quibus comparantur inter ſe bina tempora immerſionum, vel emerſionum, in quibus ubi alterum ad alterum reducitur, ad habendam differentiam temporis, quidquid erratur in definienda differentia longitudinum binorum locorum, erratur itidem in illa quæſita differentia temporum, per quam deinde Solis parallaxis eſt definienda. Porro longitudinum differentia multo difficilius accuratè definitur, quanquam illud commode accidit, quod ubi

ſemel ſatis accurate inſtituta fuerit in binis locis obſervatio immerſionis, vel emerſionis Veneris, differentia longitudinum poterit deinde per obſervationes, quotieſcunque libuerit, repetitas inveſtigari.

24. Ut autem ex ipſa mappa definiantur loca ad hanc obſervationem maxime idonea, pro prima methodo ſeligenda ſunt ea loca, in quibus duratio phænomeni eſt maxima, et minima; pro ſecunda, et tertia ſeligenda ſunt loca, in quibus immerſio, vel emerſio accidet celerrime, et tardiſſimè; verum ejuſmodi loca ſunt ſeligenda, ad quæ accedi poſſit, ut in iis ſatis commode liceat inſtituere obſervationem.

25. Durationem omnium breviſſimam invenio prope Tobolſkam in Siberia, ubi, neglectis ſecundis, eſt proxime horarum 5, 58′; eſt autem Petropoli, et Pekini 5^h 59′. Berolini 6^h 0′. Longiſſimam omnium circa auſtralem incognitam plagam in longitudine 110° circiter, ac latitudine 60°, ubi eſt proxime 6^h 13′. Si eo adire liceret, haberetur diſcrimen cum Tobolſka minutorum ferè 15; ſed ea regio adiri utique non poteſt. Sunt loca in Nova Hollandia ad maris littus, ubi duratio eſt 6^h 6′, quod adhuc exhibet cum Tobolſka differentiam minutorum 8, ſed nulla eſt ſpes eo appellendi. E locis, quæ ſpem aliquanto majorem appulſus exhibeant, Batavia præbet durationem 6^h 4′, quod pro differentia exhibet cum Tobolſka minuta 6, et 5 cum Petropoli, et Pekino, quibus in locis obſervationes exactæ intra duo, vel tria ſecunda adhuc exhiberent diſtantiam Solis, et parallaxim intra centeſimam ſui partem accuratas.

26. Immerſio omnium citiſſimè videri incipiet circa Tropicum Capricorni in longitudine 235° in Mari Pacifico hora 2 40′, omnium autem locorum maxime-

maxime ſero prope Cyprum inſulam hora 2 57′, quæ duo loca differentiam exhiberent minutorum 17. In ea error decem etiam ſecundorum adhuc relinqueret Solis parallaxim et diſtantiam definitas intra centeſimam ſui partem. Et in Cypro quidem obſervatio admodum facile inſtitui poteſt, ac erat, uti monui, in Gallia, qui profectionem meditaretur; verum plura alia ex eadem parte ſunt loca fere æquè apta, in quibus obſervationes non deerunt. Adero ego Conſtantinopoli, ubi habetur hora 6 56′. Habentur Berolini minuta pluſquam 55, Stokholmiæ, et Petropoli fere accuratè is numerus, Tobolſkæ 54½, in quibus locis omnibus obſervatores utique habebuntur. At alter locus Mari Pacifico ſitus non niſi inſulas habet omnino deſertas longiſſimè à Continenti Americæ ſitas ad diſtantiam graduum pluſquam 60. Is, qui aliquanto facilius id commodi Aſtronomiæ procurare poſſet, eſt, ut arbitror, ſolus Hiſpaniæ Rex, qui quidem Ulloam Aſtronomum egregium, et Quitenſibus obſervationibus celeberrimum, nunc habet in Peruvia, unde in aliquam ex iis inſulis navigare poſſet, et accuratam obſervationem inſtituere, ſine qua quidquid Petropoli, Stokholmiæ, Berolini, Conſtantinopoli, in Cypro inſulo fiet, erit omnino irritum, et ſine ejuſmodi obſervatione nihil omnino deduci poterit ſecunda hac methodo, ut prima etiam methodus ſine Batavica obſervatione eſt ferme prorſus inutilis.

27. Relinquitur methodus tertia, quæ ad emerſionem pertinet. Loca, in quibus ea primo apparebit, ſunt paullo ultra Kamtchkatkam ad extremos Orientalis Aſiæ fines, ubi id phænomenum accidet hora 8^h 49′; loca, in quibus poſtremo videri deſinet, ſunt in ipſa incognita Auſtrali plaga circa primi Meridiani latitudinem

dinem graduum 45, ubi desinet hora 9 6′, differentia existente iterum 17 minutorum, ut in methodo secunda. Sed licet eo deveniri non possit, adest insula S. Helenæ aptissima sane, in qua id accidit fere $9^h\ 4\frac{1}{2}'$, et linea, quæ exhibet $9^h\ 4'$ transit prope Fernambucum in Orientali Brasiliæ ora situm, fere pervadit extremam ultimæ meridionalis Africæ oram, ac transit per ipsum caput Bonæ Spei, quæ idcirco loca omnia, si cum Kamchkatka conferantur, exhibent plus, quam 14 minuta discriminis. Verum cum habeatur finis Pekini hora fere 8 51′, Tobolskæ $52\frac{1}{2}'$, Petropoli 54′, Berolini 55′, Londini 56′, Constantinopoli $56\frac{1}{2}'$; observatio habita in insula S. Helenæ cum hisce locis collata exhibet differentiam majorem minutis 13, 12, 10, 9, $8\frac{1}{2}$, 8, quarum priores ad rem maximè idoneæ sunt, penultima etiam admodum opportuna, si illud consideretur, plures Londini observationes futuras, et quidem omnino accuratissimas.

28. Triplex autem generatim haberi potest errorum fons; primus in dirigendo horologio, vel horologii ipsius æquabilitate; secundus in determinando momento immersionis, vel emersionis; tertius in determinanda positione loci, in quo observatio ipsa instituitur. Ex primo capite non video, quid timeri possit, cum intra eundem diem communia etiam horologia oscillatoria, si bona sint, æquabilitatem retinere soleant potissimum circa Solstitia, circa quæ minus varia Cœli constitutio esse solet, et per altitudines correspondentes Solis et fixarum repetitas horologium satis facile ita derigatur, ut ne unius quidem secundi error inde timeri possit. Saltem adhibitis horologiis accuratissimis, et peculiari

diligentia in obſervando, omnis error inde oriundus evitari commode poteſt.

29. Ex ſecundo capite juxta ea, quæ initio diximus, major quam duorum ſecundorum error meo quidem judicio in ſingulis obſervationibus timeri omnino non poteſt; quin immo infra unicum etiam ſecundum deprimi poteſt adhibitis teleſcopiis ejuſdem vis. Quamobrem, ſi hæc adhibeatur diligentia, in caſu maximè infauſto, in quo bini errores ſe mutuo nequaquam corrigunt, nonniſi duorum ſecundorum error timeri poteſt.

30. Et hi quidem ſoli ſunt errores, qui in prima methodo timeri poſſint, licet duplicandi ibi ſint idcirco, quod et immerſionis et emerſionis obſervationem ea methodus requirat, at pro reliquis videndum præterea, quid timeri poſſit, in determinanda poſitione locorum, in quibus obſervationes inſtituuntur. Determinatio latitudinis loci nec requiritur admodum accurata, et admodum facile accuratiſſima obtinetur per culminantium fixarum altitudines, potiſſimum ſi aliquanto majoris radii quadrantes adhibeantur. Longitudinum determinatio eſt aliquanto difficilior. Eam per eclipſes ſatellitum Jovis, non credo, obtineri poſſe ſine periculo erroris plurium ſecundorum; adhuc tamen, ſi teleſcopia adhibeantur ejuſdem vis, et plures ejuſmodi obſervationes inſtituantur, credo utique intra ſex horaria ſecunda poſſe etiam per ipſas differentiam longitudinum definiri. Verum ſi occurrant fixarum occultationes per Lunam, vel aſſumantur per egregium objectivum micrometrum plures diſtantiæ Lunæ, à fixis cognitis, iiſdem autem diebus accurate alicubi, ut Londini, et Pariſiis, obſervetur et tranſitus Lunæ per meridianum, et diameter

meter apparens Lunæ, à qua pendet parallaxis quoque, credo utique, intra quatuor etiam horaria ſecunda, ingenti habito obſervationum numero, rem poſſe concludi.

31. Hinc eruitur pro primo methodo ſperari poſſe determinationem intra quatuor, vel etiam duo ſecunda, pro reliquis binis intra ſex, vel quinque, atque id etiam, ſi, quod fere nunquam evenit, errores omnes in unam ſummam coaleſcant. Quare ſi in prima methodo habeatur obſervatio inſtituta Tobolſkæ, et in Nova Hollandia, ubi vidimus differentiam expectari minutorum 8, ſperari poteſt determinatio parallaxeos intra centeſimam vigeſimam, vel ducentiſimam quadrageſimam partem totius; ſi autem habeatur obſervatio Tobolſkæ, et Bataviæ, ubi differentia expectatur minutorum ſex, poſſe ſperari determinationem intra nonageſimam, vel centeſimam octogeſimam partem totius.

32. Pro ſecunda methodo, ſi habeatur obſervatio illa in inſulis Maris Pacifici circa loca, quæ ſupra vidimus, licet, ut recedatur à limitibus, quibus exiguo calculi errore loco motis, fruſtrari poteſt obſervatorem ſua ſpes, amittatur unum minutum in tempore, ac ea obſervatio conferatur cum obſervationibus ibidem nominatis, Cypria, Conſtantinopolitana, Berolinenſi, Petropolitana, Stokholmienſi, Tobolſkæana, ubi differentia haberi debet minutorum 16, 15, 14, $13\frac{1}{2}$, admiſſa etiam errorum ſumma ſecundorum 6, haberi poteſt determinatio intra partem centeſimam ſexageſimam, vel centeſimam quinquageſimam, quadrageſimam, trigeſimam quintam totius.

33. Pro tertia methodo idem fere fructus ſperari poteſt, ſi obſervatio inſtituatur in inſula S. Helenæ,

vel Fernambuci in Brafilia, vel Occidentalem Africæ oram, uti ex iis, quæ ſupra diximus patet. Obſervatio autem habita in inſula S. Helenæ, et comparata cum Londinenſi, et Conſtantinopolitana exhibentibus differentiam minutorum $8\frac{1}{2}$, et 8, promittunt determinationem intra octogeſimam quintam, et octogeſimam partem totius.

34. Binæ quidem tantummodo obſervationes accuratæ, et inſtitutæ in locis ſatis idoneis, rem omnino conficiunt, at plura ſunt, quæ jubeant plures hac illac obſervatores dimittere; primo enim collatis pluribus obſervationibus inter ſe, apparebit, intra quos limites certi eſſe poſſimus de parallaxi Solis eruta, videndo, quantum ejuſmodi determinationes à ſe invicem diſtent, ac aſſumendo medium certiores erimus, et vero valori propriores: tanti utem momenti eſt id, quod hic quæritur, ut nihil, quod ad majorem accurationem, et certitudinem conferre poſſit, omitti debeat. Deinde cavendum, ne nubes aſpectum præripiant, quod quidem nequaquam accidet, ſi multi obſervatores in locis fuerint admodum diverſis. Et hinc ego quidem cum in meo poemate de eclipſibus agerem ante hos octo annos de hoc ipſo Veneris ſub Sole tranſitu, et Halleyanam reſpicerem methodum, illud adjeci.

Credo equidem denſæ circum ſe quaque cohortes
Effundent latè terris, quaque horrida triſti
Squalet humus glacie, quaque uſtis ardet arenis,
Nec tantas imo nubes alit Æolus antro,
Ventorumque ciet gens improba, poſſit ut omnes
Obruere aërios tractus, atque omnibus axem
Ætherium velare locis, et condere Divam.

Fors et ego Irroquiûm ſedes, et inhoſpita regna,
Si tantum fortuna ſinet, ſi vita ſuperſtes,
(Quid namque hæ vires, quid non hæc ſpondeat ætas,
Dum vix ter quartus ter denis additur annis?)
Tranſcurram procul et doctis comes ibo catervis.
Spero equidem, nec me pontus turgentibus undis
Audentem terret, glacieſque niveſque perennes.

35. Et ego quidem commutata nunc obſervationum ineundarum ratione non in Americam Septentrionalem, ſed Conſtantinopolim abibo, ubi obſervationem inſtituam cum iis inſtrumentis, quæ mihi præſto erunt, non quidem optima, quæ maxime cuperem, ſed ad rem utcumque perficiendam idonea: labori certè et diligentiæ non parcam. Et ea quidem obſervatio maximum ſecunda methodo habebit uſum, ſi habeatur obſervator aliquis in iis, quas innuimus, Maris Pacifici inſulis; aliquem autem adhuc non contemnendum in tertia methodo, ſi habeatur obſervator aliquis in inſula S. Helenæ, vel in ora Occidentali Meridionalis Africæ partis, vel Fernambuci in Braſilia.

36. Quid autem Anglicana natio, et Regia potiſſimum Londinenſis Societas, præſtare poſſit, ſatis conſtat ex iis, quæ diximus. Maxime omnium videtur deſideranda expeditio in inſulam S. Helenæ, ex qua habebitur optatus fructus tertia methodo. Verum ſi æmulæ nationi Gallicæ nolit palmam concedere in nobiliſſima facultate promovenda, opus eſt plures obſervatores dimittat in plures plagas, et ad alias obſervationes obtinendas alibi ſua officia interponat.

37. Pro ipſa tertia methodo poteſt utique curare bina obſervationum binaria facienda cum inſtrumentis egregiis,

egregiis, et inter ſe collatis. Ex altera parte præter obſervationem in inſula S. Helenæ, dimittendus eſſet aliquis alius vel illam Africæ oram maritimam, vel cum bona Luſitani Regis gratia Fernambucum: ex altera parte habebit omnino Societas obſervationes ſuas Londinenſes.

38. Quoniam tanti à ſecunda, et prima methodo fructus jure ſperantur, poſſet utique Regalis Societas ad Legatum Anglicum Matritenſem literas dare, ut Regem per ſe etiam amantiſſimum et bonarum artium et ſcientiarum omnium, ſuo nomine impelleret ad præcipiendam Ulloæ obſervationem illam in Mari Pacifico, deſtinata ad illum maturè tranſvehendum navi, quod quidem aulæ Hiſpanicæ eſſet admodum facile, Literariæ autem Reip. exoptatiſſimum. Poſſet et ab Hollandis obſervationem expetere, vel in Nova Hollandia, vel Bataviæ peragendam, vel etiam aliquem è ſuis eo deſtinare, petita ab Hollandis facultate. Eo pacto Gallos in hanc occaſionem intentos vinceret Anglia, ac ipſi Literarius orbis deberet exoptatiſſimam determinationem omnibus tribus methodis factam. Pudebit ſane ſuorum avorum nepotes venturo ſæculo, ſi tantam et adeo raram opportunitatem neglectam viderint, et cum ingenti Aſtronomiæ damno prætermiſſam eo potiſſimum tempore, quo maritimi ſucceſſus maritimas expeditiones etiam literarias uſque adeo faciliores reddiderant, et omnino expoſcebant.

In ſignum animi addictiſſimi
et obſequentiſſimi,
Rogerius Joſeph Boſcovich, S. I.

LXXVIII. *A Propoſal for diſcovering the Annual Parallax of* Sirius; *by the Rev.* Nevil Maſkelyne, *A. M. Fellow of* Trinity *College*, Cambridge, *and F. R. S.*

Read June 26, 1760. THE Royal Society having come to a reſolution to ſend perſons of ability to proper places, in order to obſerve the approaching paſſage of Venus over the Sun, the 6th of June next year; (which phænomenon was firſt propoſed to this Society, by the late excellent Dr. Halley, forty-four years ago, as a proper means of determining the Sun's parallax, to a great degree of exactneſs) I take this opportunity of recommending to the conſideration of this learned body another very important object in aſtronomy, which, I apprehend, may be cleared up at the ſame time, by the aſtronomers ſent to one of thoſe places, which will probably be judged convenient for the obſervation of Venus's tranſit; I mean the iſland of St. Helena.

The aſtronomical object, in favour of which I deſire to engage your attention, is no leſs than the determination of the annual parallax of the Orbis Magnus; the finding out of which, from obſervation, would be the fulleſt and directeſt proof of the Copernican ſyſtem, as the want of this proof, hitherto, has been the ſtrongeſt argument made uſe of by thoſe, who have with-held their aſſent to an hypotheſis, which ſo fully, and yet ſimply, ſatisfies all the other phænomena.

No

No one, indeed, at this time of day, will venture to aſſert, that, even if no annual parallax could be found, after the greateſt exertion of human art and induſtry, the Copernican ſyſtem was not, therefore, true; ſince the quantity of this parallax may be ſo ſmall, as to eſcape the reach of our ſight, though aſſiſted to the utmoſt. But though the defect of it would be no juſt argument againſt the Copernican ſyſtem, yet the actual demonſtration of it, from obſervation, would be a direct and convincing proof of the truth of that ſyſtem.

It remains then to be conſidered, what hope there is now left, after aſtronomy has been brought to ſuch a great degree of perfection, of being able to find out an annual parallax in any of the fixed ſtars.

I am ſenſible I may here ſeem to be preſumptuous, in venturing to treat upon this ſubject, after the many accurate obſervations made by the learned Dr. Bradley, with an inſtrument conſtructed for this very purpoſe. No one can have a higher veneration than I have for the diſcernment and exactneſs of this illuſtrious aſtronomer, whoſe merit I have the greater opportunity of being acquainted with, by having the honour of calling him my friend. I would juſt beg leave to take notice, that the ſtars, which this aſtronomer obſerved, were ſuch only as lay within a few degrees of his zenith: and though his obſervations do not ſeem to ſhew a ſenſible parallax in any of them, yet we cannot thence abſolutely conclude, that, amongſt the great number of viſible ſtars, there are none in which it may be perceptible, till they have all of them, eſpecially thoſe of the greateſt luſtre, been obſerved in proper places, near the zenith, with the like

like care and accuracy, which he has uſed: for, as Dr. Bradley has himſelf remarked, where any ſtars are remote from the zenith, the uncertainties of refraction, and the irregular motions of the air, become ſo great, as to take away from us all hopes of obſerving them to an equal degree of exactneſs.

The particular ſtar, which, I propoſe, ſhould be carefully obſerved, with a view of diſcovering its annual parallax, if ſenſible, is Sirius, the brighteſt of all the ſtars in the firmament, and which is therefore, probably, the neareſt to us of them all. With us, this ſtar paſſes the meridian, at the altitude of 22°, where the refractions are too irregular to admit of our diſcovering a very minute quantity, by obſervation: but, at the iſland of St. Helena, Sirius paſſes only half a degree ſouth of the zenith; and, on this account, I have, for ſome time, looked upon that as the moſt proper place to make obſervations at, for this purpoſe.

I ſhall now beg leave, with great deference, to lay before you a particular argument, which has, for theſe two years, induced me to think it probable, that the annual parallax of Sirius is not ſo ſmall, as to elude the nice diſcernment of our modern aſtronomers; and ſhall be glad to be determined by your opinion, whether it affords a ſufficient preſumption to us, to undertake a careful and aſſiduous ſeries of obſervations of the diſtances of Sirius, from the zenith of the iſland of St. Helena. This argument is drawn from an examination, which I have made, of the obſervations of the zenith diſtances of Sirius, taken at the cape of Good Hope, in the years 1751 and 1752, by that excellent aſtronomer, and diligent

obſerver, the Abbé de la Caille, recited in his Fundamenta Aſtronomiæ, printed about two years ſince, wherein alſo are contained all the obſervations, which he has taken of the principal fixed ſtars; a work truly worthy of its name! Every particular obſervation of the ſame ſtar is reduced to one epoch, that of the beginning of the year 1750, by applying the equations of aberration, preceſſion, and deviation, to the obſerved places; ſo that the places corrected ought all to agree together, if the obſervations were perfectly exact, and the ſtar was affected with no ſenſible motion, that was unaccounted for. Being ſatisfied with the excellence of the obſervations, I was tempted to examine thoſe of ſome of the principal fixed ſtars, in hopes of diſcovering ſome ſenſible differences in the obſervations made at different times of the year, when a parallax, if there had been any, would have had the greateſt effect. But I found very few ſtars, the obſervations of which were ſufficient in number, or taken at proper ſeaſons of the year, to give room for any inference at all. Fortunately, however, the obſervations of the zenith diſtances of Sirius were more in number, and, what is ſtill of more conſequence, in this caſe, made in various, and ſome in oppoſite ſeaſons of the year: and, upon looking them over, I was agreeably ſurprized to find a very ſenſible difference in the obſervations made at different times, agreeing in direction with what a parallax ought to produce, the zenith diſtance of Sirius in July coming out no leſs than 8″ greater than in the oppoſite ſeaſon of the year, namely in December and January: the zenith diſtance alſo in March and

April

April being of an intermediate quantity, as it ought to be: for, Sirius being in conjunction with the Sun in June and July, it is evident he muſt be then farther from the Earth than in December and January, when he is in oppoſition to the Sun, and, conſequently, his latitude and declination, which are both ſouth, muſt be leſs in the former caſe, than in the latter; and therefore, as he paſſes north of the zenith at the cape of Good Hope, his zenith diſtance muſt be greater in the former ſeaſon, than in the latter, as the obſervations indicate: but, in March and April, when Sirius is in quadrature with the Sun, and equally diſtant from the Sun and Earth, his apparent latitude and zenith diſtance muſt be the ſame as the true, or that which would obtain, if the Earth was tranſlated to the Sun, and conſequently, a mean between the zenith diſtances in July and December, agreeably to the obſervations.

I ſhall now lay before you the obſervations themſelves, together with the calculations, which I have made of the values of the parallax for each, the maximum being aſſumed 9″, which I find will beſt reconcile the obſervations with one another.

			Zenith distance of Sirius at Cape, observed and reduced.			Annual parallax to reduce obſervat. to mean.	Zenith distance corrected for annual parallax.			Difference between each obſervation corrected, and the mean of the diſtances corrected.
			°	′	″	″				
1751.	July	12.	17	31	19.1	— 4.3	17	31	14.8	— 1.1
	July	13.	17	31	19.3	— 4.3	17	31	15.0	— 0.9
	July	14.			21.7	— 4.3			17.4	+ 1.5
	July	15.			19.7	— 4.3			15.4	— 0.5
	July	16.			19.0	— 4.3			14.7	— 1.2
1752.	April	5.			16.7	— 1.0			15.7	— 0.2
	April	3.			15.7	— 0.8			14.9	— 1.0
	March	28.			14.0	— 0.4			13.6	— 2.3
	March	14.			13.1	+ 0.7			13.8	— 2.1
	March	8.			15.2	+ 1.2			16.4	+ 0.5
	March	5.			18.8	+ 1.4			20.2	+ 4.3
	March	4.			17.3	+ 1.5			18.8	+ 2.9
	March	1.			15.9	+ 1.6			17.5	+ 1.6
	Feb.	17.			12.8	+ 2.6			15.4	— 0.5
	Jan.	21.			11.1	+ 4.0			15.1	— 0.8
1751.	Dec.	31.			11.6	+ 4.5			16.1	+ 0.2
Mean of zenith diſtances corrected for parallax - - - - - - -							17	31	15.9	

The firſt column ſhews the year and day of the obſervations; the ſecond the zenith diſtances of Sirius at the cape of Good Hope, as delivered in Abbé de la Caille's recital; the third contains the computed values of the parallax at different times, taking that of the maximum 9″; the fourth column gives obſervations reduced to the mean, by applying the parallax computed in the third column to the obſervations in the ſecond; which quantities ought all to agree together, if the obſervations were liable to no error, and the parallax was rightly aſſumed: but, taking a mean of them all, the laſt column ſhews how much each of them differs from that mean,

mean, which, in general, is very ſmall, and ſcarce exceeds 2″, except in two obſervations, in one of which it amounts to 3″, and the other to 4″: but theſe differ as much from the mean of the ſix other obſervations made at the ſame ſeaſon of the year. Thus, aſſuming a parallax, the obſervations will be found to agree as well with that ſuppoſition, as they do with one another. But if the obſervations are conſidered in themſelves, without any allowance for parallax, they will differ ſenſibly from one another: nor is this difference to be found only in two obſervations, in which caſe, it may eaſily be attributed to the account of the unavoidable errors; but five obſervations in July oppoſed to two in December and January, make the zenith diſtances vary 8″ in the direction, which a parallax ought to produce.

I am aware, that it may be objected, that two obſervations made in the winter ſeaſon in December and January, at one of the maxima of the parallax, are too few to determine a point of ſuch conſequence, and readily agree, that the argument is weakened in proportion to the paucity of the obſervations: but then, it ſhould alſo be conſidered, that the obſervations made in March and April concur with the reſt in ſupporting the ſuppoſition of a parallax; and, upon the whole, the obſervations will, perhaps, be judged to afford a ſufficient preſumption of the exiſtence of a parallax, to encourage the undertaking a careful trial. It is upon the ſtrength of this preſumption, principally, that I have ventured to lay theſe thoughts before the Royal Society, whoſe members may juſtly claim the honour of having made the greateſt attempts towards demonſtrating this important, but yet undetermined point in aſtronomy.

LXXIX. *Far-*

LXXIX. *Farther Experiments in Electricity*; *by Mr.* Benjamin Wilſon, *F. R. S.*

To the Right Honourable the Earl of Macclesfield, *Preſident of the Royal Society.*

My Lord,

Read Nov. 13, 1760. IN my letter upon the *Tourmalin*, which I had the honour of communicating to the Royal Society in December laſt, there are ſome experiments to ſhew, that glaſs is permeable by electricity. I have ſince made others of the like kind, which ſeem to favour the ſame opinion.

The high regard your Lordſhip has always ſhewn for experimental philoſophy, encourages me to lay before you theſe experiments, with a few others, of a very curious nature.

But I beg leave, firſt, to take notice, that our electrical apparatus is much improved, by the diſcovery of Father *Windelinus Ammerſin*, of *Switzerland*, who, in a Latin treatiſe publiſhed in 1754, has ſhewn us, that *wood*, properly dried, till it becomes very brown, is a non-conductor of electricity *; and recommends boiling the wood in linſeed oil, or covering it all over with varniſh, after being dried, to prevent any return of moiſture into its pores. He

* It appears, from the Philoſophical Tranſactions, ſo early as the year 1747, that Dr. *Watſon* having occaſion to ſupport a long wire, in the experiment made near Shooter's Hill, with a view to determine the velocity of the electric fluid, uſed ſtakes of dry wood, (which, he tells me, *were baked)* to prevent the electric fluid from eſcaping into the ground.

adds,

adds, that wood ſo treated ſeems to afford ſtronger appearances than even glaſs *.

The following experiments were made, with the aſſiſtance of wood thus prepared.

Being provided with a large ſquare of glaſs, poliſhed on both ſides, and fixed upright upon one edge, I placed, for a conductor, a ſlender piece of ivory, about one foot long, having one end within two tenths of an inch from the center of the glaſs: at the other end were ſuſpended two ſmall balls of pith, by threads four inches long. The ivory was ſupported horizontally by a ſtand made of the *prepared wood*. When the glaſs was made a little warmer than the external air, my finger rubbed that ſide thereof which was fartheſt from, and oppoſite to, the ivory. Upon doing this, the two ſides of the glaſs were electrified *plus*, as were likewiſe the balls; which continued *plus*, even after they were removed from the glaſs into any part of the room.

From the appearance of a *plus* electricity in the ivory, as well as on the two ſides of the glaſs, the fluid muſt have flowed from the finger into the glaſs, and part of it into the ivory: otherwiſe, permit me to aſk how the ivory ſhould be electrified *plus?*

That the fluid really flowed from the finger into the glaſs, may be inferred from the following experiment.

A piece of ſilver, being fixed upon a ſlender rod of prepared wood, I rubbed the ſame glaſs with it, as I had done before with the finger; upon which the

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* See the Abbé Nollet's Letters upon Electricity, publiſhed at Paris in 1760.

ſilver was electrified *minus*, and both ſides of the glaſs, with the conductor and balls, *plus*.

There are, therefore, certain circumſtances under which the electric fluid paſſes through glaſs; I repeat in *certain circumſtances*, my Lord, becauſe in others, for inſtance, the *Leyden bottle*, the fluid does not paſs through the glaſs, but electrifies one ſide *plus*, and the other ſide *minus*, as my friend Dr. *Franklin* hath ſhewn, in his letters upon electricity.

This difference, which is cauſed by the vitreous electricity in both caſes, may perhaps be explained from the *ſurrounding medium* *, and the different degrees of the *power* with which the electric fluid endeavours to enter a body.

For the *reſiſtance* owing to the *ſurrounding medium* in a poliſhed plane, without covering, is not overcome, but by bringing the *power* near the glaſs, whereby the glaſs is electrified *plus* on both ſides; whereas, when the glaſs is *properly covered*, and the *power* brought upon it, the covering conducts the fluid equally to all the parts on that ſide, where it accumulates, that is, electrifies it *plus*; whilſt the oppoſite covering carries off to the communicating earth an equal quantity of the fluid naturally belonging to the glaſs, that is, electrifies it *minus*.

The ſame glaſs, without any covering, will be rendered *minus* on both ſides, by removing the *power* to a greater diſtance, by which it is diminiſhed, and the quantity of intermediate air, and conſequently of *reſiſtance*, increaſed.

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* See the former paper upon the Tourmalin, p. 329. of this Volume, Part I.

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The *power* therefore which cauſes the *minus* effect, is leſs than the *power* which cauſes the *plus* effect: and that this *minus* effect ariſes intirely from the *repulſive power* of the fluid, appears from this, that part of the fluid naturally belonging to the glaſs, is driven off, and electrifies *plus* other bodies placed beyond it.

From hence I collect, that the *three different* effects, *viz.* the electrifying glaſs *plus on both ſides*; or *plus on one ſide*, and *minus on the other*; or, laſtly, *minus on both ſides*; are occaſioned by the *different degrees* of the *ſame power* and *reſiſtance* in the reſpective experiments with the *ſame glaſs*.

If, on the other hand, the three laſt experiments are made with the *minus*, inſtead of the *plus* electricity, the appearances, and conſequently the reaſoning upon them, are inverted: for the *plus* appearance, in that caſe, muſt be owing to the tendency of the fluid from the earth, air, and other neighbouring bodies, towards the oppoſite ſide of the glaſs, where the *power* acts, which cauſes the *minus* appearance.

This regular converſion ſerves to confirm the truth of the principles aſſigned.

And here, with your Lordſhip's leave, I ſhall ſet down ſome farther experiments, moſt of which have been made with a view to ſhew, that a *plus* electricity may be produced by means of a *minus* electricity.

Having electrified the inſide of a large *Leyden* bottle *plus*, by means of a conducting-wire from an excited glaſs globe, I ſet it upon a ſtand of prepared wood, and took away the conducting-wire; after which, the mouth of the bottle was cloſed with a

ſtople of glaſs. Then the pointed end of the ivory conductor was brought oppoſite to the middle of the bottle, and about two inches from it. Upon doing this, the balls were electrified *minus*; and more ſo, as the ivory was moved nearer the bottle in an horizontal direction.

But, on removing the ivory to a greater diſtance, the *minus* electricity decreaſed; and, at a certain diſtance, there was not any ſign thereof remaining: and when the diſtance was increaſed to about eighteen inches from the bottle, a *plus* electricity appeared, which continued, even after the ivory was removed intirely away.

After unelectrifying the bottle, it was ſet again upon the ſtand of wood; and upon another of the ſame kind, was laid a large iron poker, ſo that the pointed end was about two inches from the outſide coating, and much nearer the bottom than the top of the bottle. I choſe the lower part thereof, for no other reaſon, than to have the poker out of the reach of any effect that might happen from the conducting-wire, which communicated with the machine, and the inſide coating of the bottle. The other end of the poker, being a large round knob, was about one inch from a rod of iron, which communicated with the earth. Under theſe circumſtances, when the inſide of the bottle was electrified *plus*, the outer coating became *minus*, as uſual.

It is to be obſerved here, that the *plus* appearance in the poker was cauſed by that portion of the fluid, which was driven off from the outſide of the bottle, by the repulſive force of the fluid conveyed into the bottle. And this confirms the reaſoning upon the remarkable

markable experiments related in the treatiſe publiſhed by Dr. Hoadly and myſelf. See p. 27 to 34, and 46, 47.

Inſtead of electrifying the inſide of the bottle, in the laſt experiment, *plus*, I electrified it *minus*, with the prepared wood, without any other change of circumſtances. In this caſe, the outſide of the bottle was *plus*, as uſual, and the poker *minus*. The fluid, therefore, which cauſed the *plus* electricity on the outſide of the bottle, muſt, in part at leaſt, have flowed from the poker; becauſe there was no other open communication with the outſide of the bottle and the earth to ſupply it.

With a cylinder of baked wood, which ſhewed ſtronger ſigns of electricity, than I have yet been able to produce from glaſs, the balls hanging to the ivory were electrified *minus*, at the diſtance of four feet, or more, by holding the cylinder over the middle of the ivory, and continuing it there: and on moving it nearer, they were more ſtrongly electrified *minus*; but the ſame cylinder, on removing it back again to the diſtance of two or three feet, or more, electrified the balls *plus*.

When another conductor, for inſtance metal, without edges or points, was uſed inſtead of the ivory, and without any thing hanging from it, the ſame cylinder held over the metal, as was done in the laſt experiment over the ivory, at the diſtance of two feet, produced a *plus* electricity: and this was rendered weaker, as the cylinder was moved nearer; but by leſſening the diſtance to about one foot, the *minus* electricity took place.

In theſe caſes, the *plus* appearance aroſe from the earth, air, or other neighbouring bodies.

When the preceding experiments were firſt made, I was a little embarraſſed with the uncertain appearances of a *plus* electricity at one time, and a *minus* at another, in the ſame experiment: but, by repeated trials and obſervations, I have found that a *plus* or *minus* electricity may be produced at pleaſure, by carefully attending to the following circumſtances, *viz.* the form of the bodies, their ſudden or gradual removal, and the degrees of electrifying.

I ſhall now proceed to acquaint your Lordſhip with ſome other circumſtances of as nice a nature, where the ſlighteſt, and almoſt imperceptible differences in the *poſition*, or in the *force* of the *friction*, of two bodies, produce in either of them, the *plus* electricity at one time, and the *minus* at another.

Such are the effects of this ſubtile and active fluid, when the experiments are carefully made; and therefore, they require the moſt ſcrupulous attention, to trace out the cauſes which occaſion them.

Sealing wax and ſilver were the bodies uſed in the firſt two experiments; but many other ſubſtances ſeem to perform as well. The ſealing wax was clean, and undiſturbed by any friction whatever, but that of the air ſurrounding it; and had been ſo for ſome hours. The ſilver was fixed to a piece of the prepared wood, which was alſo preſerved from friction for the ſame length of time. Then taking one of theſe ſubſtances in each hand, the ſilver being at that end of the wood the fartheſt from the hand, I laid the ſmootheſt part of the ſilver upon the ſealing wax, and moved it along the ſurface gently, *once only*, and with a very *ſlight preſſure*; after which, the ſilver was electrified *plus*, and the wax *minus*.

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On repeating the experiment, with equal care, and in the ſame manner, except that the *ſmooth ſide* of the ſilver was a little inclined, ſo that the *edge* thereof preſſed againſt the wax; the ſilver, after moving it as before, was electrified *minus*, and the wax *plus*; which is contrary to what was obſerved in the laſt experiment.

Theſe oppoſite effects, occaſioned by the different applications of the flat, or edge of the ſilver, ſeem to ariſe from an alteration made in the ſurface of the wax, by *deſtroying* the *poliſh* in one caſe, and not in the other: and, in this reſpect, reſembles the poliſhed and rough glaſs mentioned in the letter upon the Tourmalin. See p. 328. of this Volume, Part I. Experiment 24.

Upon making uſe of prepared wood, inſtead of wax, and employing different degrees of preſſure in the friction, with the *ſame edge* of the ſilver, I produced the like appearances; the leaſt preſſure cauſing a *plus*, and the greateſt preſſure a *minus* appearance in the ſilver.

A flat piece of ſteel, well poliſhed, and the edges rounded off, afforded the ſame appearances, by only applying the *flat ſurface* to the wood; but it required more *preſſing* to produce the *minus* effect in this caſe, than it did in the former, where the *edge* was concerned.

Whether the reaſon offered above for explaining theſe laſt curious appearances be true or not, I do not venture to affirm, for want of farther experiments; but thus much may be ſafely advanced, that we have learned to produce at pleaſure the *plus* or *minus* electricity from the ſame bodies, by attending to the manner of their *application* and *friction*.

Mr. Dufay, and the Abbé Nollet, have indeed obſerved an inconſtancy in the effects, when they rubbed ſealing wax; but for want of the cautions above-deſcribed, in the choice and treatment of the bodies, they were not able to anſwer for the event of any experiment. The Abbé, in particular, who has taken remarkable pains to find out from whence this uncertainty ariſes, acquaints us, in the *thirteenth letter*, with his difficulties, and how much it perplexed him to ſee, within an hour, in the ſame place, and with the ſame inſtruments, the effects changed, and become altogether contrary to thoſe he had before obſerved. In another part of the ſame letter, he ſays:

" Je me ſuis flatté plus d'une fois d'avoir ſaiſi les " circonſtances qui faiſoient tourner à coup ſûr le ré- " ſultat de l'experience d'un côté ou de l'autre; mais " la ſuite m'a fait connoître que je m'étois trompé, " ou qu'il manquoit encore quelque choſe à ma dé- " coverte."

I might add other quotations of the ſame kind, but, as it would take up too much of your Lordſhip's time, I ſhall beg leave to refer to the letter itſelf.

From the experience now obtained, it may not be improper to obſerve farther, that ſealing wax of different hardneſs, will occaſion a difference in the effects, the hardeſt being always *plus*, and the ſofteſt *minus*; which agrees very well with the rule laid down in the letter upon the Tourmalin, p. 331, " that where electric appearances are produced by " the rubbing of any two poliſhed bodies together, " that body, whoſe ſubſtance is hardeſt, and electric " power ſtrongeſt, will be always *plus*, and the ſofteſt " and weakeſt always *minus*." Now we find that the

the Dutch wax is ſofter than the Engliſh, and the Iriſh harder: and it is not improbable that the French wax, which I ſuppoſe the *Abbé Nollet* uſed, may likewiſe be different.

Whoever, therefore, may be deſirous of trying the preceding experiments, with a view to make them ſucceed, as they are here ſet down, muſt uſe the *beſt Engliſh ſealing wax*.

I ſhall conclude with an experiment made by my friend *Mr. Hamilton*, profeſſor of philoſophy in the univerſity of Dublin, as it ſeems to illuſtrate the doctrine of *reſiſtances*, at leaſt, ſo far as reſpects the air.

Let a ſlender braſs, or iron wire, five or ſix inches long, and finely pointed at each end, be fitted in the middle, with a braſs cap, void of angles; then let half an inch at each extremity be bent in oppoſite directions, till they are perpendicular to the reſt of the wire, and in ſuch a manner, that when the wire is ſuſpended, by means of its cap, on a point of metal, it may lie in a plane parallel to the horizon. The pointed metal, which ſupports this wire, muſt be two or three inches long, and have its other end fixed into a ſmall block of wood. Now, if this block, with a wire ſuſpended, be ſet upon an electrified body, the wire will turn round with a very great velocity, moving always in a direction contrary to that in which the electric fluid iſſues from its points, without having any conducting ſubſtance near it, ſave that of the air: and if the wire be made to turn round by any other force, in the oppoſite direction, ſo that its points go foremoſt, it will, when electrified, ſoon be

be deprived of that motion, and made to turn round the contrary way.

This experiment, he ſays, was contrived, in order to try whether the electric fluid, which iſſues ſo freely from pointed bodies, would have any effect to move theſe bodies by its reaction; and that it has ſuch an effect, ſeems ſufficiently manifeſt from the event.

Mr. Hamilton apprehends, that the electric particles, by their elaſtic force, iſſue directly forwards from the points, and endeavour to expand themſelves; but meeting with ſome reſiſtance from the air, force the wire to move backward in a contrary direction, much in the ſame manner that a Catherine-wheel is made to turn round in a direction, contrary to that in which the ſmall rockets affixed to its periphery diſcharge themſelves. And therefore, he is inclined to think, that it might be made uſe of as an electrometer, by having it to turn round in a plane perpendicular to the horizon, and loading the wire with ſmall weights near one of its extremities, which will be raiſed to a greater diſtance from the perpendicular line, as the electric fluid is ſtronger.

I am, my Lord,

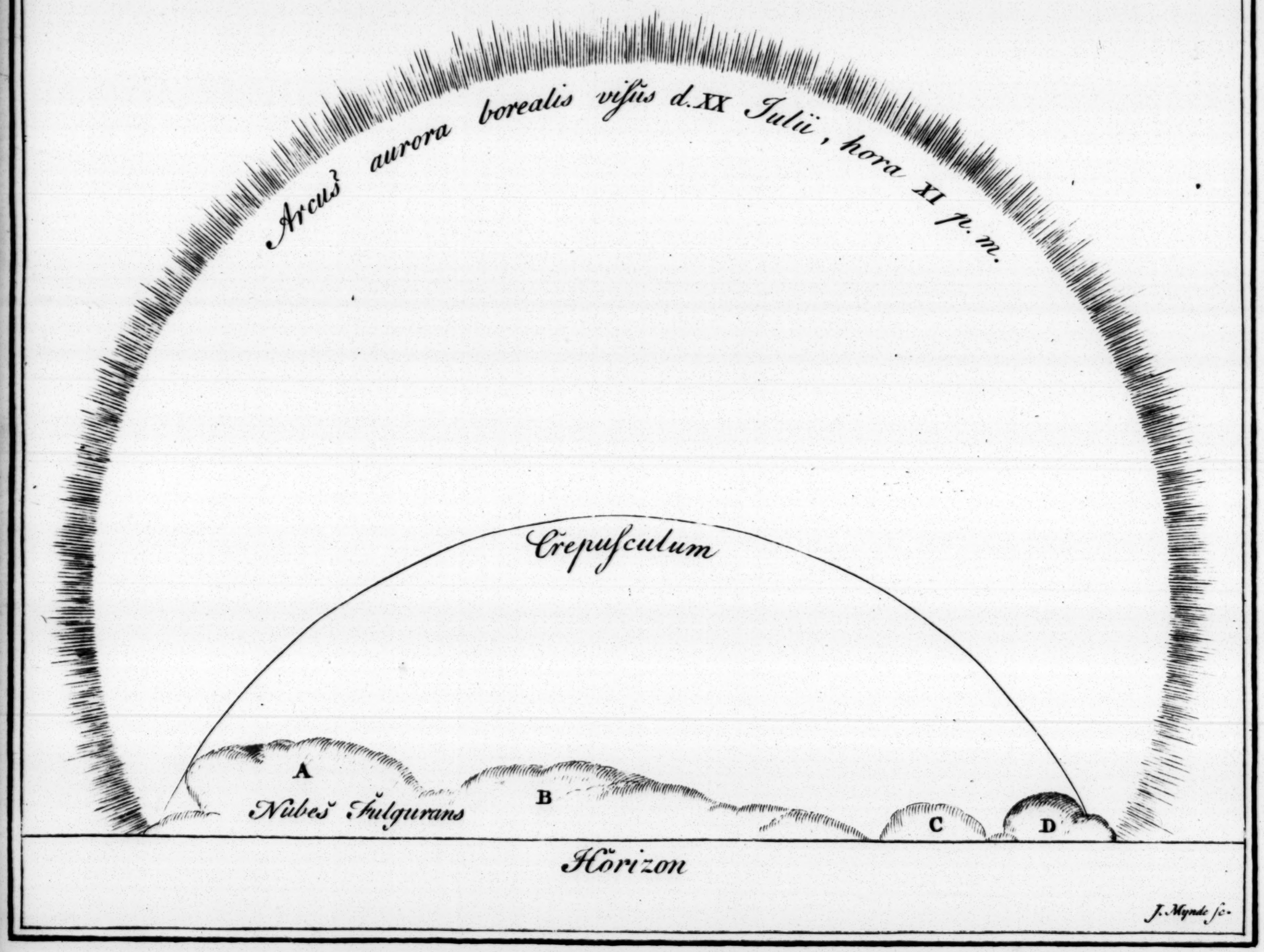
Your Lordſhip's

moſt obliged

and obedient ſervant,

Benj. Wilſon.

Great Queen-Street,
Nov. 5, 1760.


Arcus aurora borealis visus d. XX Julii, hora XI p. m.
Crepusculum
A
Nubes fulgurans
B
C
D
Horizon
J. Mynde sc.

LXXX. *A Letter to Mr.* Benjamin Wilſon, *F. R. S. concerning Electricity*; *from Mr.* Thorbern Bergman, *of* Upſal.

Ampliſſime atque Celeberrime Domine,

Read Nov. 20, 1760. NON mireris, Vir Celeberrime, me phyſicis addictum Scientiis tuam quærere amicitiam, cujus nomen jam diu in hoc Scientiarum genere orbi erudito inclaruit. Præterea Dominus Profeſſor Ferner me certiorem fecit Te non ægre laturum mecum *commercium literarium* inſtituere de *rebus electricis, aliiſque curioſis phyſicis.* Plurima enim apud Vos quotidie inveniuntur, quorum tempeſtiva cognitione lætatur quivis Philoſophus naturalis; forte etiam varia apud nos prodeunt, quæ Anglis haud ingrata ſpero. Itaque conſtitui ſecundum tuum nutum, eos Philoſophiæ naturalis progreſſus, quos *in Suecia* capit, notos facere, à Te viciſſim expectans eorum notitiam, quibus *in Anglia* locupletatur. Lingua utor Romana, quippe quæ mihi notior Anglica, cujus genius ab exterraneo, qui in Anglia nunquam fuit, vix ita addiſci poteſt, quin multa vitia irrumpant, quæ Anglo inter legendum nauſeam movent.

Dominus *Franklin in* §. 28. *tertiæ epiſtolæ miratur glaciei fruſtum non tranſmittere commotionem electricam,* cum *aqua idem perfecte præſtet:* ſed feci nonnulla experimenta, quæ monſtrant *parvam aquæ quantitatem nec huic fini ſufficere.* Scilicet capiatur tubus vitreus trium vel quatuor pedum, qualis barometris inſervit: hic aqua impleatur, et claudatur utrumque extremum ſubere perforato filo metallico aquam paulum intrante, uti figura adpoſita indicat.

A——A

Si jam duo homines in circulo explosorio constituti teneant extrema filorum metallicorum A, A, et tentetur evacuatio, non tamen perfecta evenit, nam vix carpus et rariſſime flexura cubitus hac methodo concutitur, licet deinde idem vas absque ulteriori cumulatione tantum contineat electricitatis, ut more consueto evacuatum pectus valide ferire possit. Aucta tubi capacitate, magis transit, usque dum ita increverit, ut plenarium transitum permittat, nec impedit aquæ quantitas justo major, quod testantur commotiones per lacus et flumina propagatæ. *Conveniunt itaque aqua et glacies in eo, quod parvæ quantitates concussionem ægre tramittant.*

Hinc suspicatus sum *magnam glaciei quantitatem faciliorem concedere transitum, quod etiam experientia comprobavit*, etsi adhuc *non majus* adhibui *frustum* quam quo *commotio flexuram cubitus attigit.* Præterea *ex æqualibus* aquæ et glaciei quantitatibus, *glacies minus transmittit.*

Num *Auroras Boreales* uti *phænomena electrica* respicias, nec ne, nescio; *mihi* saltem adhuc *incertum* videtur. Interea *singularem observationem* æstate præterlapsa factam commemorabo. *Die xx Julii hora x. post meridiem vidi in N. W. nubem* A B *horizonti insistentem, in qua copiosæ et crebræ fulgurationes* ab A versus B progrediebantur, *absque* tamen ullo *tonitru*, ne quidem minimo murmure. C et D erant nubeculæ rariores forte cum magna non cohærentes, nam in eis nulla vidi fulgura. [*Vide Tab.* XXI.] *Hora xi circiter adparuit Aurora Borealis* suo *culmine ultra zenith* progrediens, et pluries *arcus extremitatibus nubem attingens.* Etsi probe examinarem, nullam

nullam aliam communicationem inter nubem et auroram obſervare potui, nam 1°. in D, cui inſtitit alterum crus, nullam vidi fulgurationem: 2°. dum in nube fulguraret auroræ ſplendor, nullam variationem pati videbatur. Hora xii deſierant fulgura, et nubes ita erat dilatata, ut totum fere hemiſphærium viſibile occuparet.

Præterito, et *hoc quoque anno, multas elegantiſſimas obſervari Auroras Boreales.* Si quiſpiam *tales Londini* attentis conſideraverit oculis, per tuum favorem *notitiam* me obtenturum ſpero, et *quando et quomodo* ibi apparuerint, ut ſciam num fuerint eædem, quas hic vidi. Saltem obſervationum collatione certius hujus phænomeni altitudo perpendicularis determinaretur. Si tibi placuerit, alia occaſione lubenter meas tranſmittam obſervationes.

Interea favori tuo me commendo permanſurus

Ampliſſimi atque Celeberrimi Nominis Tui

Cultor obſervantiſſimus,

Dabam Upſalæ,
17 Oct. 1760.

Thorbern Bergman.

Quomodo electricitas cumulari poſſit in aliis quam viteris materiis, ex. gr. lacca ſigillatoria, ſulphure, papyro, ære, &c. adeo ut commotio electrica ex eis obtineatur, non haud ita pridem in actis Academiæ Scient. Stokholmenſis docuit Carolus Vilcke Phyſices Lector; Methodus forte tibi non ignota.

LXXXI. *Some Conſiderations on a late Treatiſe intituled,* A new Set of Logarithmic Solar Tables, *&c. intended for a more commodious Method of finding the Latitude at Sea, by Two Obſervations of the Sun*; *by* H. Pemberton, *M. D. R. S.* Lond. *et R. A.* Berol. *S.*

Read Nov. 20, 1760.

AS it happens not unfrequently at ſea for the unſeaſonable intervention of clouds to prevent the ordinary method of determining the ſhip's latitude by the ſun's meridian altitude, even when it is of primary conſequence, that the true latitude ſhould be known; a problem for remedying this diſappointment is ſtated in many treatiſes of navigation, for finding the latitude of a place by any two altitudes of the ſun, with the interval of time between them.

A problem ſimilar to this is propoſed, and ſolved inſtrumentally upon a globe, by a very early writer, Petrus Nonius (*a*), namely, to find the latitude by two altitudes of the ſun, and the angle made by the azimuth circles paſſing through the ſun, when the altitudes are taken. And ſince more commodious and accurate inſtruments for meaſuring time have been invented, than were known to this author, the other problem has been propoſed for the ſame pur-

(*a*) In Libr. De Obſerv. Regul. et Inſtrum. Geometr. lib. ii. c. 14.

poſe,

poſe, of which a conſtruction upon the principles of the ſtereographic projection of the ſphere is exhibited by Mr. Collins, in his Mariner's Plain Scale new planed (*b*). And as the direct method of ſolving both theſe problems by numbers requires a diverſity of trigonometrical operations, a ſet of tables has lately been publiſhed for a more compendious way of computation in the problem, where the interval of time is given, whereby the ſhip's true latitude may be very expeditiouſly derived from the ſhip's dead reckoning, provided the obſervations are made within certain limits of time.

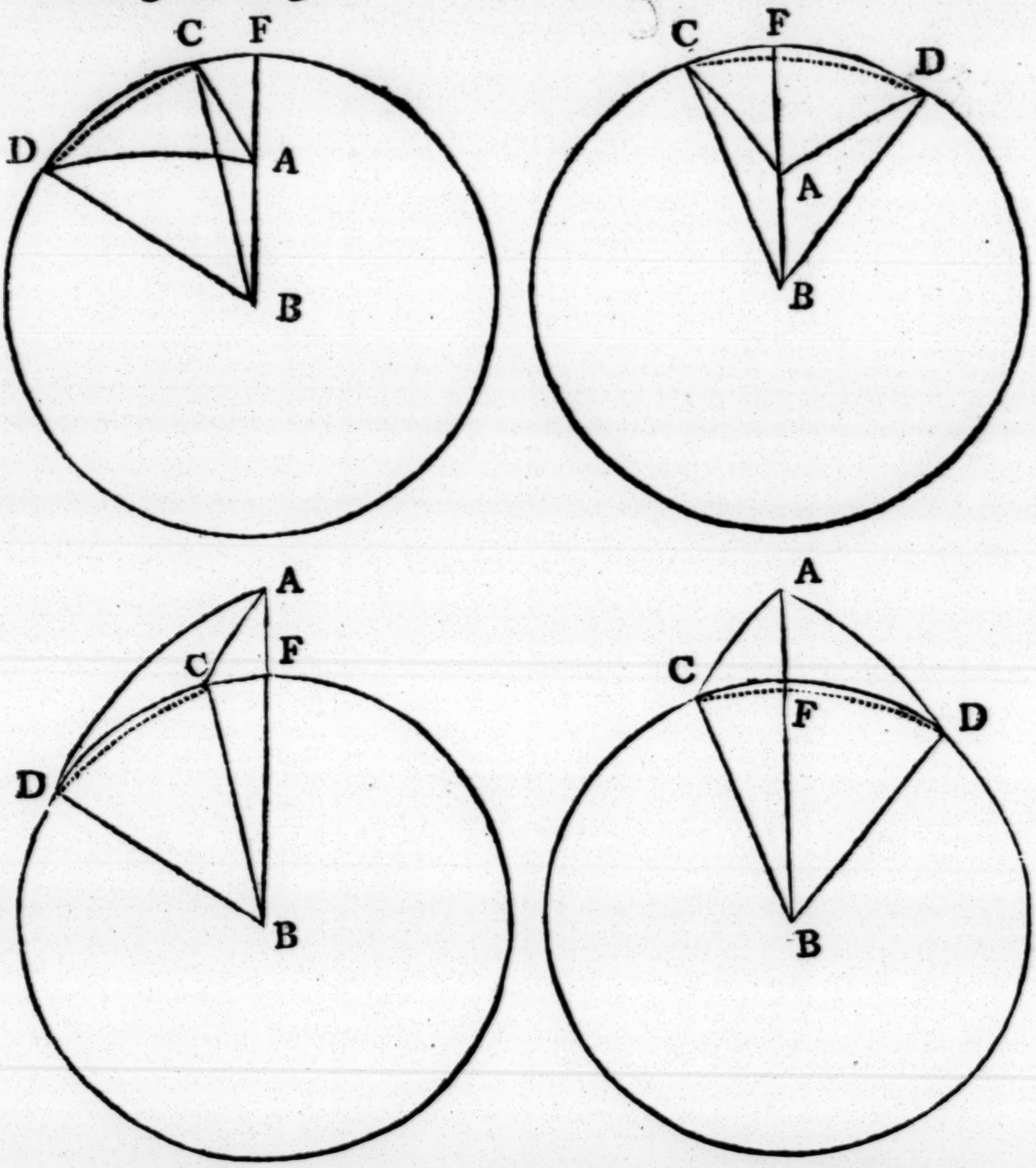
But however worthy of notice this method may be, new tables for the purpoſe are altogether unneceſſary. It conſiſts of two parts: the firſt computes, from the latitude exhibited by the dead reckoning of the ſhip, the diſtance from noon of the middle time between the obſervations, and thence the time of either: the ſecond operation computes, from one of theſe obſervations, what ſhould be the ſun's meridian altitude, had the ſhip's reckoning given the true latitude; but if the latitude aſſumed from that reckoning is erroneous, the altitude thus computed will not be conformable to it; however, if the times for the obſervations are properly choſen, it will much better agree to the true latitude, and thereby the aſſumed latitude may be more or leſs corrected.

But both theſe operations are an immediate conſequence from the propoſition in ſpherical trigonometry, uſually delivered under the name of the fourth axiom, which is this; That the ſquare of the radius is to the rectangle under the ſines of the ſides con-

(*b*) Book iii. p. 35

taining

taining any angle, as the verſed ſine of that angle to the difference between the verſed ſines of the third ſide, and of the difference between the ſides contain-taining the angle.



Hence if A be the zenith of any place; B the elevated pole; C, D two places of the ſun in the parallel of declination D F; by this propoſition in the triangle A B C,

Rad.^{q} : ſin. AB × ſin. BC (coſ. lat. × coſ. decl. ⊙) :: verſ. ſ. ABC : verſ. ſ. AC — verſ. ſ. AF;

And, in like manner, in the triangle ABD,

Rad.^{q} : coſ. lat. × coſ. decl. ⊙ :: verſ. ſ. ABD : verſ. ſ. AD — verſ. ſ. AF;

Conſequently,

Rad.^{q} : coſ. lat. × coſ. decl. ⊙ : : verſ. ſ. ABD — verſ. ſ. ABC : verſ. ſ. AD — verſ. ſ. AC.

But $\frac{1}{2}$ rad. × $\overline{\text{verſ. ſ. ABD} - \text{verſ. ſ. ABC}}$ is = ſin. $\frac{\text{ABD} + \text{ABC}}{2}$ × ſin. $\frac{\text{ABD} - \text{ABC}}{2}$;

Therefore,

Rad.^{q} : coſ. lat. × coſ. decl. ⊙ :: ſin. $\frac{\text{ABD} + \text{ABC}}{2}$ × ſin. $\frac{\text{ABD} - \text{ABC}}{2}$: $\frac{1}{2}$ rad. × $\overline{\text{verſ. ſ. AD} - \text{verſ. ſ. AC}}$.

And this is the firſt operation in the treatiſe, theſe remarks concern. For in the rectangle, the ſin. $\frac{\text{ABD} + \text{ABC}}{2}$ × the ſin. $\frac{\text{ABD} - \text{ABC}}{2}$, the firſt ſide, when the obſervations are both made on the ſame ſide of noon, and the ſecond ſide of this rectangle, when one obſervation is before noon, and the other after, is the ſine of the diſtance of the middle time between the obſervations from noon, and the other ſide is the ſine of half the diſtance between the obſervations. And in the new tables one column exhibits the arithmetical complement of the logarithmic ſine of half the elapſed time, and the next, the logarithm of twice the ſine of the middle time.

But moreover, $\frac{1}{2}$ rad. × $\overline{\text{verſ. ſ. AD} - \text{verſ. ſ. AC}}$ being equal to ſin. $\frac{\text{AD} + \text{AC}}{2}$ × ſin. $\frac{\text{AD} - \text{AC}}{2}$, the operation may be expreſſed more commodiouſly thus,

Rad.

Rad.^{q} : cof. lat. × cof. decl. ⊙ :: fin. $\frac{ABD+ABC}{2}$ × fin. $\frac{ABD-ABC}{2}$: fin. $\frac{AD+AC}{2}$ × fin. $\frac{AD-AC}{2}$; now requiring the common logarithmic fines only.

Again, the fquare of the radius being, as above, to cof. lat. × cof. decl. ⊙, as the verf. f. ABC to verf. f. AC — verf. f. AF, or as verf. f. ABD to verf. f. AD — verf. f. AF, thefe analogies expref the fecond operation, which, in this treatife, is unneceffarily confined to the leffer angle ABC. The column in the table intituled Rifing confifts of logarithmic verfed fines, which may be applied to either of the angles ABC or ABD promifcuoufly; for here AF being equal to the excefs of BC or BD above the arch BA, affumed for the complement of the latitude, the arches AC and AD, in the preceding analogies, will be the complements of the altitudes obferved, if the latitude were truly affumed, otherwife not; but the difference of their verfed fines, will however be equal to the difference of the verfed fines of the complements of the true latitudes; for this is fuppofed in the firft operation. Therefore, if one of the fourth terms of thefe analogies be deducted from the verfed fine of the complement of the greater altitude, or the other from the verfed fine of the complement of the leffer, the remainder will be the fame, and exhibit a verfed fine for the complement of the fun's altitude different from AF, when BA is affumed different from the true latitude, and nearer to the truth, if the times for the obfervations are properly chofen.

But farther, the two preceding analogies may be reduced to thefe;

Rad.

Rad.^{q} : cof. lat. × cof. decl. ⊙ :: $\overline{\text{fin. } \frac{1}{2} \text{ABC}}^q$: $\frac{1}{2}$ rad. × $\overline{\text{verſ. ſ. AC} - \text{verſ. ſ. AF}}$,

And

Rad.^{q} : cof. lat. × cof. decl. ⊙ :: $\overline{\text{ſin. } \frac{1}{2} \text{ABD}}^q$: $\frac{1}{2}$ rad. × $\overline{\text{verſ. ſ. AC} - \text{verſ. ſ. AF}}$;

Alſo,

Verſ. ſ. AC − verſ. ſ. AF = cof. AF − cof. AC, and verſ. ſ. AD − verſ. ſ. AF = cof. AF − cof. AD.

And thus the common tables of ſines will ſupply the uſe of this column of the new tables, as well as the preceding.

The firſt example of this treatiſe, wrought by the common tables.

Here the declination of the ſun is ſtated at 11° 17′ N. the altitude firſt taken, being the leſſer, 46° 55′, the greater altitude 54° 7′, and the difference of time between the obſervations $1^h\ 25^m$, or 85^m; which, divided by 4, gives 21° 15′ for the arch of the equinoctial correſponding.

Here the obſervations are both before noon; therefore, in fig. 1. the latitude being aſſumed 46° 50′,

For the firſt operation.

	°	′	″	
AC =	35	53	0	
AD =	43	5	0	
Their ſum =	78	58	0	
Their difference =	7	12	0	
Sine of $\frac{1}{2}$ ſum	(39	29	0)	9.80336
Sine of $\frac{1}{2}$ difference	(3	36	0)	8.79789
Arith. com. of the ſine of $\frac{1}{2}$ CBD (=	10	37	30)	0.73429
Arith. com. of the ſine of BF, or the cof. of the decl. of the ſun, - - - - - - - - - - - - - -				0.00848
			Sum	19.34402
			Cof. of the lat. aſſumed	9.83513
Diff. = ſin. $\frac{\text{ABD} + \text{ABC}}{2}$				9.50889

For the ſecond operation.

$\frac{ABD+ABC}{2}$ = 18 49 50	
$\frac{ABD-ABC}{2}$ = 10 37 30	
ABC = 8 12 20	
Sine of ½ ABC (4 6 10)	8.85458
	8.85458
Cof. of decl. ⊙	9.99152
Cof. of the lat. aſſumed	9 83513
Sum − 30.00000 = log. of the $\frac{\text{verſ.ſ. AC} - \text{verſ.ſ. AF}}{2}$	7.53581
Verſ. ſ. AC — verſ. ſ. AF	0.006868
Natural cof. of AC, the complement of the greater altitude, - - - - - - - - - - - - - -	0.810212
Natural cof. of 35° 12′ 23″	0.817080

Or, $\frac{ABD+ABC}{2} + \frac{ABD-ABC}{2}$ being = ABD,

Sine of ½ ABD (14° 43′ 40″)	9.40522
	9 40522
Cof. of the decl. of the ſun	9 99152
Cof. of the latitude aſſumed	9.83513
Sum − 30.00000 = log. of the $\frac{\text{verſ. ſ. AD} - \text{verſ. ſ AF}}{2}$	8.63709
Verſ. ſ. AD — verſ. ſ. AF	0 086720
Natural cof. of AD, the complement of the leſſer altitude, - - - - - - - - - - - - - -	0.730361
Natural cof. of 35° 12′ 23″, as before	0.817081

Thus the meridian diſtance of the ſun from the zenith is computed at 35° 12′ 23″, and this added to the ſun's declination 11° 17′ N. gives 46° 29′ 23″ for the latitude, different from that aſſumed, and nearer to the true latitude; which, by a ſecond operation

operation with this latitude now found, will approach ſtill nearer to the truth, thus:

The ſum of the four firſt logarithms in the firſt of the two former operations, - - - - - - - - -		19.34402
Coſine of	46° 29′ 23″	9.83789
Sine of $\frac{ABD + ABC}{2}$ =	18 42 25	9.50613
$\frac{ABD - ABC}{2}$ =	10 37 30	
ABC =	8 4 55	
$\frac{1}{2}$ ABC =	4 2 27½	8.84800 8.84800 9.99152 9.83789
Log. $\frac{\text{verſ. ſ. AC} - \text{verſ. ſ. AF}}{2}$		7.52541
Verſ. ſ. AC — verſ. ſ. AF		0.006704
Natural coſ. of AC, as before,		0.810212
Natural coſ. of 35° 13′ 21″		0.816916

The declination of the ſun added to this, amounts to 46° 30′ 31″, the true latitude, as directly computed, being 46° 30′ 19″.

However, it muſt not be expected, that this method of computation will always converge thus expeditiouſly to the true latitude. Had theſe obſervations been made about 3 hours ſooner in the day, and with the ſame interval between them, if the firſt altitude of the ſun had been found 18° 55′, and the ſecond 33° 11′, the latitude computed would come out wider from the truth, than that aſſumed, one exceeding, and the other falling ſhort of the true latitude, which will lie between them, and is nearly the ſame, as above. But if the leſſer altitude had

been 29° 13′, and the greater 40°; in 9 degrees of north latitude the interval between the obſervations would have been nearly the ſame, as before. But if this latitude be ſought by any latitude aſſumed near it, the latitude computed according to the method above, will fall more remote from the truth, than that aſſumed, and err the ſame way.

In general, the error in the latitude aſſumed will bear to the error in the latitude computed, nearly the ratio compounded of that of the rectangle under the radius and the coſine of the diſtance from noon of the middle point of time between the two obſervations to twice ſin. $\frac{1}{2}$ A B C × ſin. $\frac{1}{2}$ A B D, and the ratio of rad. × ſin. A F to ſin. B C × coſ. A B; inſomuch that the diſtance of the middle point of time between the two obſervations from noon is to be conſidered, as the limit, where theſe computations ſhall ceaſe to converge, when the rectangle under the radius, and the coſine of this diſtance from noon ſhall be to twice ſin. $\frac{1}{2}$ A B C × ſin. $\frac{1}{2}$ A B D, (or the coſine of this diſtance to the difference between the verſed ſine of this diſtance from the verſed ſine of half CBD) as ſin. BF × coſ. AB to rad. × ſin. A F. And the errors in the aſſumed and computed latitudes fall on different ſides of the true latitude, when both the obſervations are on the ſame ſide of noon, and the zenith lies between the meridian ſun and the elevated pole; or when the obſervations are one before, and the other after noon, if the meridian ſun paſſes between the zenith and the elevated pole: otherwiſe, they fall on the ſame ſide of the true latitude.

However,

However, the direct method of computation, by the aſſiſtance of the natural ſines, will not be ſo much more operoſe than this compendium, as may at firſt ſight be imagined. For the arch of a great circle being drawn through C and D, forming the triangle BCD, if the logarithmic coſine of BC or BD be added to the logarithmic tangent of half the angle CBD, and the logarithmic ſine of BC or BD be added to the logarithmic ſine of half this angle, the firſt ſum is the logarithmic cotangent of the angle BCD, and the ſecond the logarithmic ſine of half CD, the baſe of the triangle. Then, in the triangle ACD, from the ſides, now all given, is to be computed the angle ACD, the difference between which, and the angle above found BCD, is the angle BCA, when the zenith lies between the pole, and the great circle through C, D; but when the zenith lies beyond that circle, the angle BCA is either the ſum of thoſe angles, or the ſupplement of that ſum to a circle. And, in the laſt place, if twice the logarithmic ſine of half this angle, and the logarithmic ſines of BC and AC are added together, the ſum, after thrice the logarithm of the radius has been deducted, is the logarithm of half the exceſs of the natural coſine of BC ~ AC above the natural coſine of AB, or the natural ſine of the latitude, according to the trigonometrical axiom, which has been above referred to; for rad. $\times \frac{1}{2}$ verſ. ſ. BCA is equal to $\overline{\text{ſin. } \frac{1}{2} \text{BCA}}^{2}$ (c).

(c) Vide Neper. Mirif. Canon. Conſtruct. Edinburg. 1619, p. 59.

The

The operation for the preceding example will be this:

Tang. $\frac{1}{2}$ CBD —	9.273225	Sine $\frac{1}{2}$ CBD —	9.265714
Cof. BC —	9.291504	Sine BC —	9.991524
Cotang. BCD —	9.564729	Sine $\frac{1}{2}$ CD —	9.257238
BCD =	87° 53′ 52″	$\frac{1}{2}$ CD =	10° 25′ 2½″

	°	′	″		
AD =	43	5	0		
AC =	35	53	0	— 0.232001	ar.co. of the fines.
CD =	20	50	5	— 0.448950	
Sum =	99	48	5		
$\frac{1}{2}$ Sum =	49	54	2½	— 9.883621	fines.
$\frac{1}{2}$ Sum — AD =	6	49	2½	— 9.074466	
			Sum	— 19.639038	
			$\frac{1}{2}$ Sum	— 9.819519	

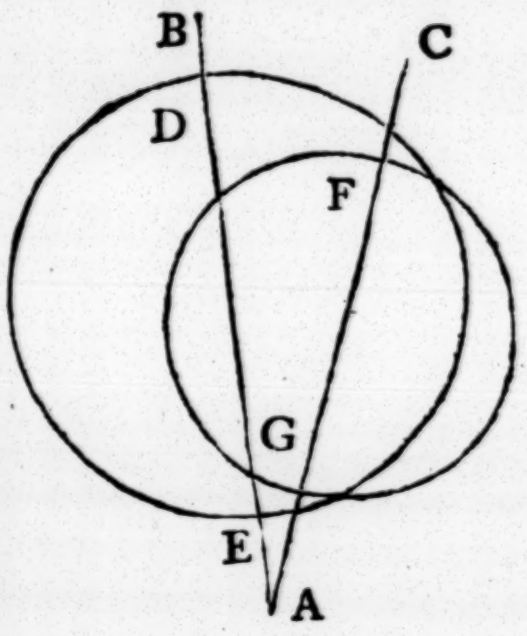
This half ſum is the logarithmic coſine of half ACD; therefore, half ACD is = 48° 42′ 11″, and its difference from $\frac{1}{2}$ BCD will give $\frac{1}{2}$ ACB = 4° 45′ 15″.

Sine of $\frac{1}{2}$ ACB	8.918444	
	8.918444	
Sine of AC —	9.767999	
Sine of BC —	9.991524	
0.0039483 — log. —	7.596411	
2		
0.0078966		

0.7333345 — natural coſine of BC — AC, that is, of 42° 50′.
0.7254379 — natural coſine of AB, or ſine of the latitude.
Therefore, the latitude is 46° 30′ 19″.

But farther, as in the caſes, where the above approximation takes place, it is an advantage, that the aſſumed latitude ſhould be taken as near the true one,

one, as can be, the dead reckoning of the ſhip need not be intirely relied on for that purpoſe; for the inſtrumental conſtruction propoſed by Mr. Collins (*d*) will very readily give the true latitude, as nearly as the inſtruments uſed can expreſs: and his method may be deſcribed thus.



Two ſtraight lines AB, AC being drawn in an angle correſponding with the diſtance in time between the obſervations, and in one of the lines, as A B, the lengths A D, A E being taken from any ſcale of tangents, one equal to the tangent of half the ſum, and the other equal to the tangent of half the difference of the diſtance of the ſun from the pole, and from the zenith of the firſt obſervation, and alſo the points F, G, taken in the other line in the ſame manner related to the other obſervation; then circles being deſcribed on the two diameters D E, F G, the diſtance of A from one of the interſections of theſe circles will be the tangent of half the complement of the latitude.

Moreover, as in the treatiſe, which has given occaſion to this diſcourſe, it is propoſed ſometimes to take into conſideration the ſhip's motion during the time, between the obſervations, but imperfectly, regard being had to the change made by the ſhip in longitude only; but the change in latitude alſo may be taken into conſideration in the foregoing con-

(*d*) In the place above cited.

ſtruction,

ſtruction, by making the angle B A C equal to the ſum or difference of the interval of time, and the change in the ſhip's longitude, according as that change is made towards the weſt, or towards the eaſt, and then drawing a line from A, not to the interſection of the circles; but ſo, that the portions of that line, terminated by each of the circles, may be the tangents of arches, whoſe difference is half the change in the latitude; which, if made from the pole, requires the portion of the line terminated at the circle, whoſe diameter is D E, to be ſhorter than the other; and the contrary is required, when the motion of the ſhip is towards the pole.

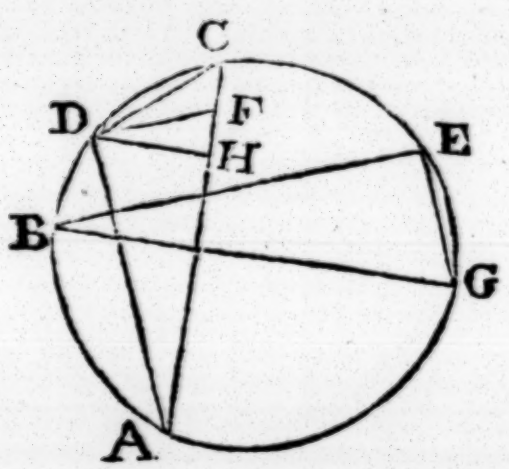
For determining the latitude by calculation, if the diſtance of the ſun in the firſt obſervation from the zenith of the ſecond could be found, this caſe would be reduced to the firſt, wherein the ſhip is conſidered, as ſtationary. And for this purpoſe, it has been propoſed to make an additional obſervation, by an azimuth compaſs, of the angle, the ſhip's courſe makes with the azimuth of the ſun, when the firſt altitude is taken; and, perhaps, the ſame angle may be found with ſufficient exactneſs from the latitude in the firſt obſervation aſſumed, whether from the dead reckoning, or from the foregoing conſtruction.

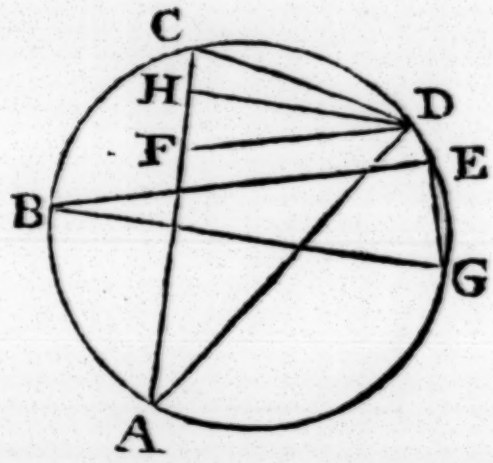
Why I have taken no notice of the calculations exhibited by the author of the piece here animadverted on, as proofs of his method, will readily appear to thoſe, who ſhall caſt their eye upon them.

Scholium.

The axiom in trigonometry, on which the calculations here diſcourſed of have been ſhewn immediately

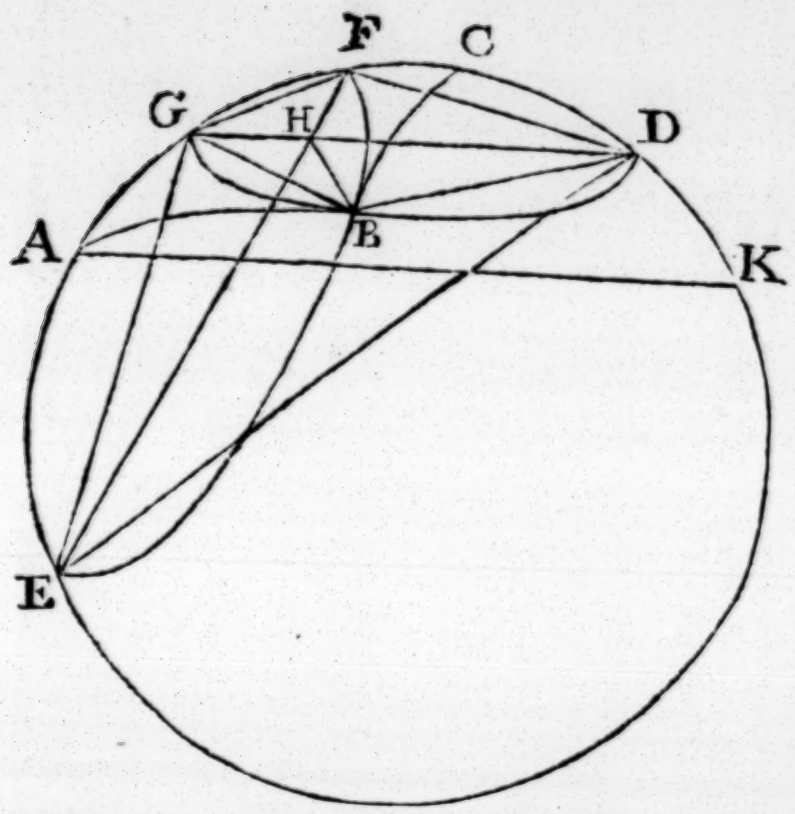
diately to depend, was introduced by Regiomontanus, and is ſtill retained, as the foundation of the preſent methods of computing logarithmically an angle from the three ſides of a ſpherical triangle given, though they may be demonſtrated more directly by the following lemma.





In the circle A B C, the chord A C being drawn, and the arch A B C biſected in B, if D be taken in the circle at pleaſure, and the lines B E, D F drawn alſo at pleaſure parallel to each other, one terminated by the circle in E, and the other by the chord A C in F; then A D, D C being drawn, B E × D F is = A D × D C.

Draw D H perpendicular to A C, and alſo the diameter B G, joining E G. Then the angles F D H, E B G are equal, and the angles D H F, B E G right. Therefore B E is to B G as D H to D F, and B E × D F = B G × D H = A D × D C.



Now, in the ſpherical triangle A B C, through B, and to the poles A, C, let the ſemicircles E B F, G B D be deſcribed, and the arch A C completed to a circle. Then the ſemicircles EBF, G B D will be both perpendicular to the plane of this circle, cut it in their diameters E F, G D, and their common interſection B H be alſo perpendicular to it, and conſequently perpendicular to G D, the diameter of the ſemicircle G B D, whereby G B, D B being joined, G D × G H is equal to G B^q, and G D × D H equal to B D^q. Again, A K being drawn parallel to G D, the arch A C K is double A C, G C D double C B, E A D the ſum of all the three ſides of the triangle, E G = E D − D G (2 C B), G F = A F (A E) + C G (C D) − A C = E D − 2 A C, and F D = E D − E F (2 A B).

Then E G, G F, F D, E D being joined; in the firſt place, A K is to D G as A K × G H to D G × G H, alſo A K to D G as A K × D G to D G^q, but A K × G H = E G × G F by the preceding lemma, and D G × G H = G B^q; whereby A K × D G is to D G^q as E G × G F to G B^q. Hence A K being twice the ſine of the arch A C, D G twice the ſine of the arch G C, equal to B C, E G twice the ſine of half the arch E A G, G F twice

twice the ſine of half the arch GF, and GB twice the ſine of half the angle ACB to the radius of the circle GBD, which is half GD; ſin. AC × ſin. BC is to rad.^{q} as ſin. $\overline{\frac{1}{2}\text{ED} - \text{BC}}$ × ſin. $\overline{\frac{1}{2}\text{ED} - \text{AC}}$ to $\overline{\text{ſin. } \frac{1}{2}\text{ACB}}^q$. And this is Napeir's firſt method of finding an angle from the three ſides given (*e*), as it is uſually delivered.

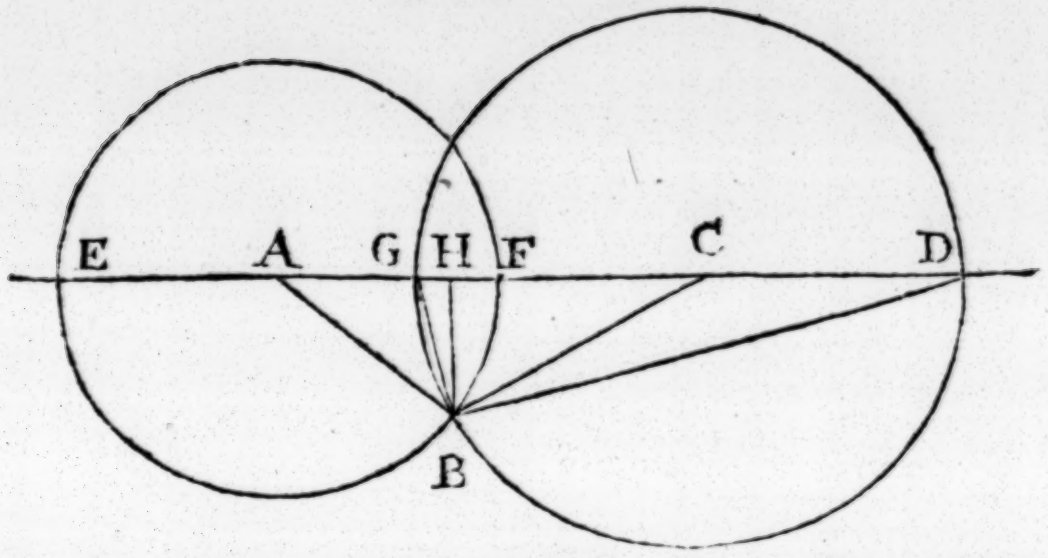
Again, AK is to DG, or AK × DG to DG^q, as AK × DH (ED × DF) to DG × DH, or DB^q. Therefore, DB being twice the ſine of half the angle BCD, or twice the coſine of half the angle ACB to the radius of the circle GBD, as ſin. AC × ſin. CB to rad.^{q} ſo is $\frac{1}{2}$ED × ſin. $\overline{\frac{1}{2}\text{ED} - \text{AB}}$ to $\overline{\text{coſ. } \frac{1}{2}\text{ACB}}^q$. And this is Napeir's ſecond method (*f*).

And laſtly, the coſine of an arch or angle being to its ſine, as radius to the tangent, ſin. $\frac{1}{2}$ED × ſin. $\overline{\frac{1}{2}\text{ED} - \text{AB}}$ will be to ſin. $\overline{\frac{1}{2}\text{ED} - \text{BC}}$ × ſin. $\overline{\frac{1}{2}\text{ED} - \text{AC}}$ as rad.^{q} to $\overline{\text{tang. } \frac{1}{2}\text{ACB}}^q$, which is a third method added by Gellibrand (*g*).

Moreover,

In plain triangles, from the three ſides given may an angle be found by a proceſs ſimilar to each of theſe, as follows:

(*e*) Mirific. Canon. Log. deſcrip. lib. ii. c. 6. § 8.
(*f*) Ibid. § 9.
(*g*) Trigon. Britan. p. 75.



In the plain triangle ABC, if on the centers A and C be deſcribed circles through B, cutting AC in E, F, G, and D, the perpendicular BH being let fall on AC, EH × HF is equal to GH × HD. Therefore HF : GH :: HD : HE, and by compoſition GF : GH :: ED : HE, and :: ED — GF : EG; alſo HF : GF :: HD : ED, and :: DF : ED — GF.

Moreover ED is = AB + AC + CB, EG = ED — 2CB, FD = ED — 2AB, ED — GF = EG + FD = 2ED — 2CB — 2AB = 2AC. Hence GF is to GH as 2AC to EG, whence GF × EG is = 2AC × GH; and DH is to ED as DF to 2AC, that is, 2AC × DH will be = ED × DF.

Thus 2AC : DG (:: 2AC × DG : DG^q) :: 2AC × GH (EG × GF) : DGH (GB^q) and AC × CB : rad.^{q} :: $\overline{\frac{1}{2}ED - CB \times \frac{1}{2}ED - AC}$: $\overline{\text{ſin. } \frac{1}{2}ACB}^q$.

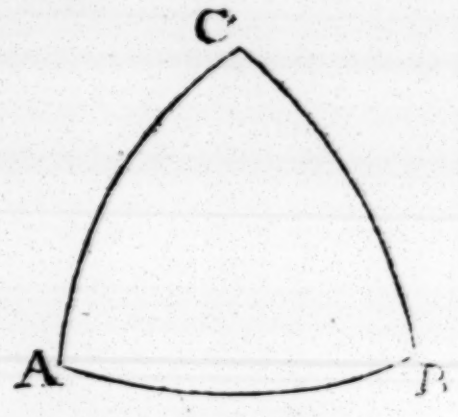
Likewiſe 2AC : DG (:: 2AC × DG : DG^q) :: 2AC × DH (ED × DF) : GDH (DB^q); therefore AC × CB : rad.^{q} :: $\frac{1}{2}ED \times \overline{\frac{1}{2}ED - AB}$: $\overline{\text{cof. } \frac{1}{2}ACB}^q$.

And in the laſt place $\frac{1}{2}ED \times \overline{\frac{1}{2}ED - AB}$: $\overline{\frac{1}{2}ED - CB \times \frac{1}{2}ED - AC}$ :: rad.^{q} : $\overline{\text{tang. } \frac{1}{2}ACB}^q$.

This

This laſt propoſition was firſt delivered by Gellibrand, and the other two not long after by Oughtred, in their reſpective treatiſes of Trigonometry.

But it may farther here be noted, that from two ſides of a plain triangle, as AC, CB, with the angle between them ACB, the third ſide AB may be found either by the firſt or ſecond of the foregoing proportions; for by the firſt may be found EG × GF, that is, $AF^q - AG^q$, and by the ſecond ED × DF, or $AD^q - AF^q$; whence by a table of ſquares AF (= AB) may be readily found, for AG is equal to AC — BC, and AD = AC + CB.



In like manner in the ſpherical triangle from the ſides AC, CB, and the angle ACB, may the third ſide be expeditiouſly found with the aid of a table of natural ſines, by the axiom referred to in the beginning of this diſcourſe, as modelled for this purpoſe by Napeir, in the propoſition, which has been already referred to (*h*), whereby the axiom is reduced to this analogy,

Rad.^{q} : ſin. AC × ſin. CB :: $\overline{\text{ſin. } \frac{1}{2} \text{ACB}}^2$: rad. × $\frac{\text{verſ. ſ. AB} - \text{verſ. ſ. } \overline{\text{AC} \sim \text{CB}}}{2}$ (*i*).

May

(*h*) Mirif. Canon. Conſtr. p. 59.

(*i*) An example of this method has been given above in the triangle ABC in p. 920. And in default of a table of natural ſines, the baſe AB might have been found thus. The ſum of twice the logarithmic ſine of ½ ACB, added to the logarithmic ſines of AC and

MAY it here be farther added, that thus all the caſes of oblique ſpherical triangles may be ſolved without dividing them into rectangular? For when two ſides and the angle included, or when two angles and the ſide between them are given, the two other angles or ſides may be found by the proportions of Napeir and Briggs, whereby the other two angles or ſides are found together (*k*); the proportions, when two ſides with the including angle are given, being theſe,

$$\text{Cof.}\ \frac{AC + CB}{2} : \text{cof.}\ \frac{AC \sim CB}{2} :: \text{cotang.}\ \tfrac{1}{2}ACB : \text{tang.}\ \frac{CAB + CBA}{2}.$$

And

$$\text{Sin.}\ \frac{AC + CB}{2} : \text{ſin.}\ \frac{AC \sim CB}{2} :: \text{cotang.}\ \tfrac{1}{2}ACB : \text{tang.}\ \frac{CAB \sim CBA}{2}.$$

The ſecond of theſe has been demonſtrated with great conciſeneſs by Dr. Halley, from the principles of the ſtereographic projection of the ſphere (*l*); and the firſt is derived by him from the ſecond, but not

---

and BC amounted to 37.596411. The logarithmic ſine of $\frac{AC \sim BC}{2}$ (9.562468) deducted from half this ſum (18.798211) leaves 9.235737. This number ſought among the logarithmic tangents exhibits for its correſpondent ſine 9.229400, which being deducted from the foreſaid half ſum (18.79211) leaves 9.568805, which is the logarithmic ſine of 21° 44′ 50½″, equal to half AB.

(*k*) Vide Neper. Mirif. Canon. Conſtr. pag. penult.

(*l*) See Jones's Synopſis, p. 281.

altogether

altogether with the ſame ſucceſs, by his not obſerving, that if the baſe and one of the ſides be completed to ſemicircles, the ſecond of the two proportions in the ſupplemental triangle thence formed leads directly to the firſt in the original triangle.

And to conclude, if two ſides, and the angle oppoſite to one of them, or two angles with the ſide oppoſite to one, were given; when the other oppoſite part is found from the proportion between the ſines of parts oppoſite, the remaining angle, or ſide, may be found by either of the two proportions foregoing.

LXXXII. *An Account of the Plants* Haleſia *and* Gardenia: *In a Letter from* John Ellis, *Eſq*; *F. R. S. to* Philip Carteret Webb, *Eſq*; *F. R. S.*

Dear Sir,

Read Nov. 20, 1760. YOU muſt have obſerved, that as the ſpirit of planting has increaſed in this kingdom, the ſtudy of botany has become more faſhionable; the works of the celebrated Linnæus, heretofore looked on as capricious and ſtrange, are now in the hands of every man, who wiſhes to ſtudy the order of nature.

The great variety of plants, which you have introduced into your garden from North America, as well as from many other parts of the world, muſt give you double pleaſure, when you view them ranged in proper order, and judiciouſly named.

The

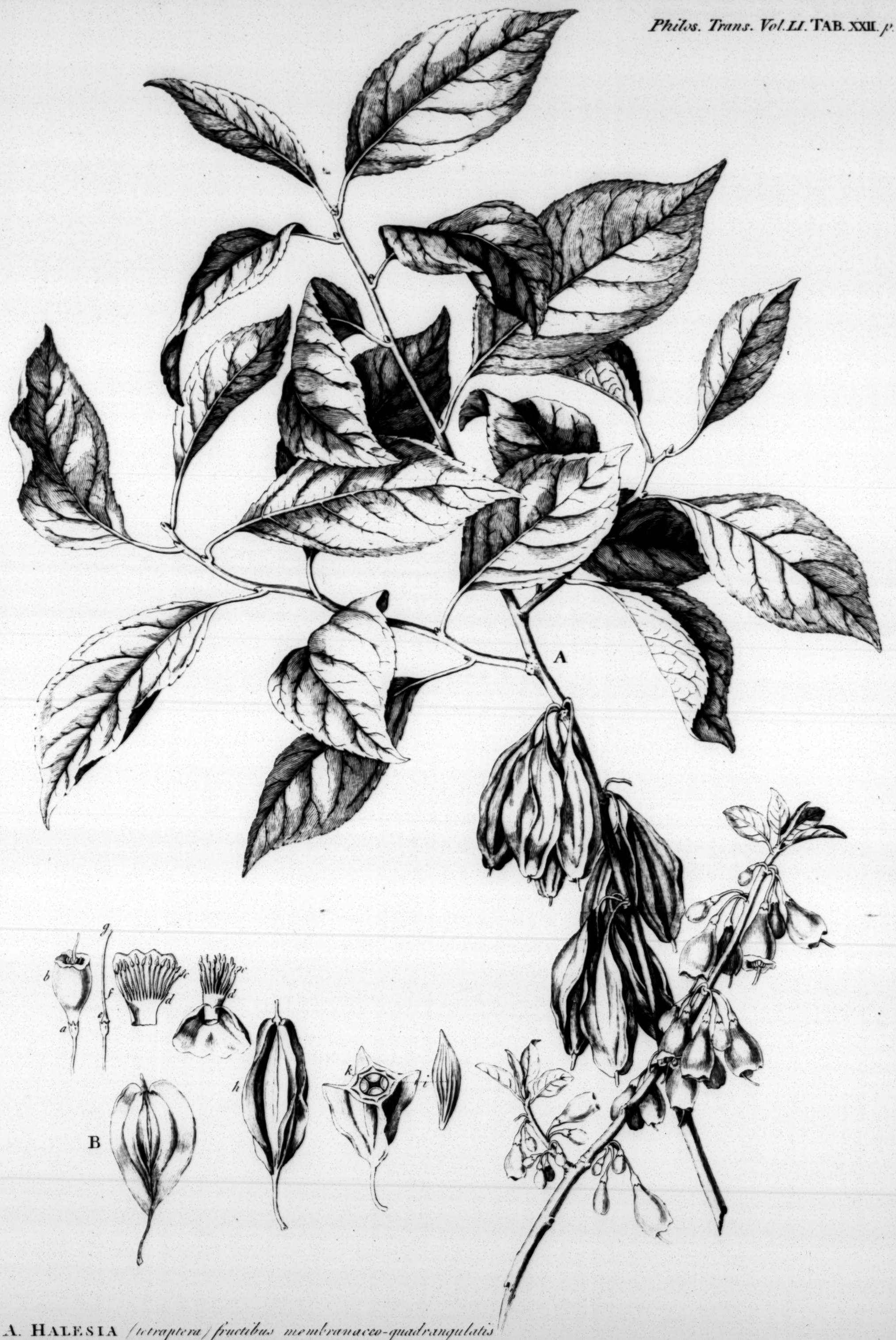
The calling of plants after the names of botaniſts, as well as after the names of thoſe, that have been, or are the promoters and encouragers of this uſeful ſcience, is not without its advantages; and this cuſtom has been followed by Linnæus, and recommended by him in his Philoſophia Botanica. It has this good effect; it ſtimulates many to the ſearching after new diſcoveries, and ſtill further improvements in agriculture and vegetation, a truly wiſe and laudable end.

The intent of this letter is to lay before you the characters of two new genera of plants, now growing in your garden, which I ſhall take the liberty to call after our worthy friends Dr. Stephen Hales of Teddington, and Dr. Alexander Garden, phyſician in Charleſtown, South Carolina.

The plant, which I ſhall firſt deſcribe, is the Haleſia; and in order to give you a better idea of it, I ſhall tranſcribe the account Dr. Garden ſent me of it, when he ſent the ſpecimens and ſeeds.

" This beautiful tree grows commonly along the " banks of Santee river, and riſes often to the ſize " of middling mulberry-trees. I have ſeen it ſome- " times more ſoutherly, near the ſmall rills of water; " but of a much ſmaller ſize than that which grows " on Santee. The wood is hard, and veined; the " bark is of a darkiſh colour, with many irregular " ſhallow fiſſures. The leaves are ovated, and ſharp " pointed, with the middle depreſſed, growing al- " ternately on ſhort footſtalks. The flowers hang " in ſmall bunches all along the branches, each gem " producing from four to eight or nine flowers, bell- " ſhaped, and of a pure ſnowy whiteneſs. As they

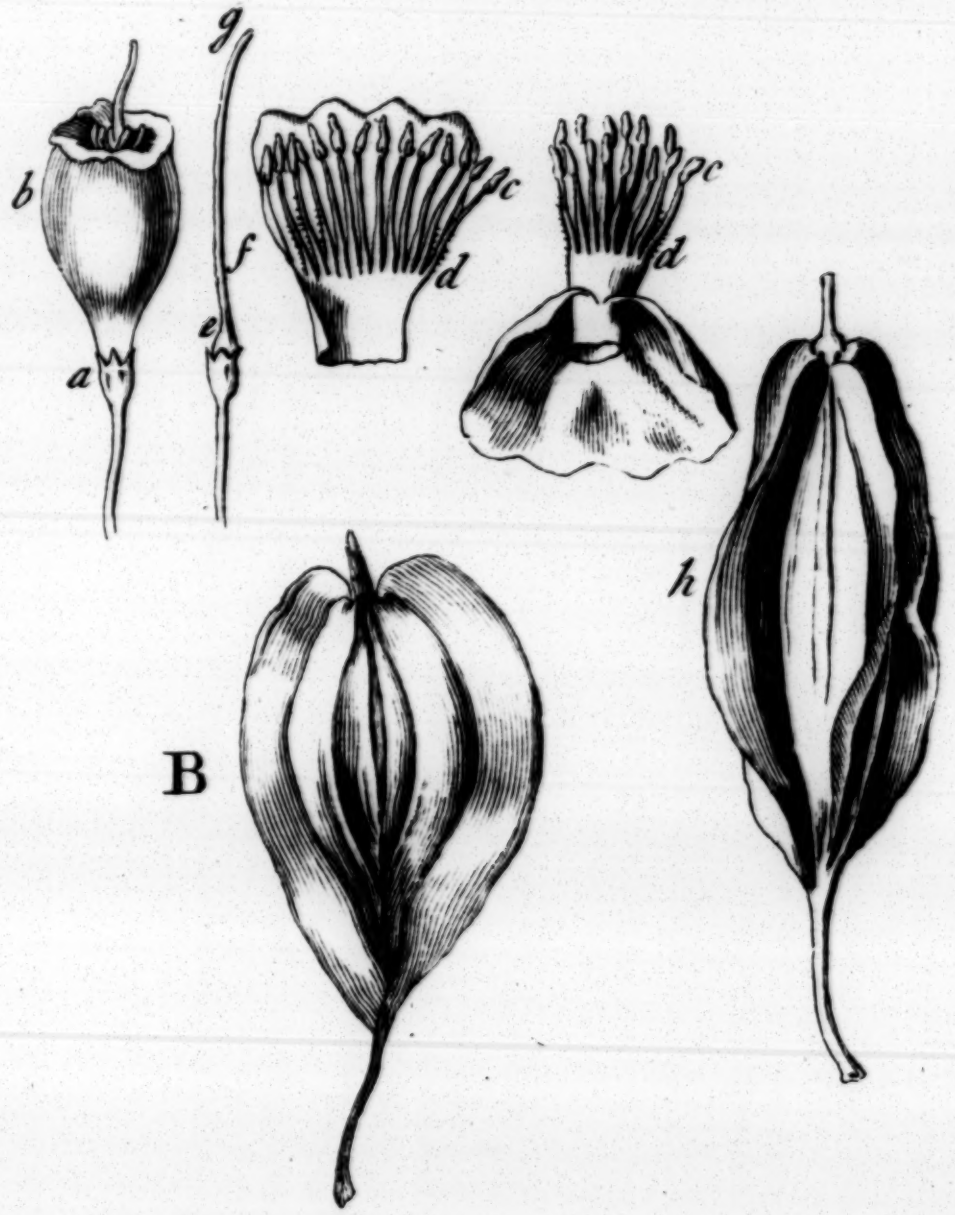
" blow



A. HALESIA *tetraptera, fructibus membranaceo-quadrangulatis*
B. HALESIA *diptera, fructibus alatis*

I. Mynde sculp.





A. HALESIA /tetraptera/ fructibus meme
B. HALESIA /diptera/ fructibus alatis

J. Mynde sculp.

" blow early in the ſpring, before the leaves appear,
" and continue for two or three weeks, they make
" a moſt elegant appearance. They are followed
" by pretty large four-winged fruit, which likewiſe
" hang in bunches, each containing four kernels,
" that are very agreeable to the taſte."

This tree is mentioned by Cateſby, Vol. I. p. 64. and called Frutex padi foliis non ſerratis, floribus monopetalis albis, campaniformibus, &c. &c.

I have not heard that it was cultivated in England, till about four years ago, when the Doctor ſent me over from Carolina a large parcel of the ſeeds, which I diſtributed among many curious gardeners, and others: but I cannot find any one that it has ſucceeded ſo well with as Mr. James Gordon, gardener at Mile-end, a man, who ſeems to be poſſeſſed of a knowlege peculiar to himſelf, in raiſing all the rarer and moſt difficult exotics from ſeeds, layers, or cuttings. He informs me, that it ſtands our winter in the open air, without any ſhelter; which confirms what Mr. Clayton of Virginia has lately wrote, that it ſtands even the ſeverity of the winter of that country. So that it may, in a little time, prove a moſt agreeable ornament to our gardens in the ſpring.

About two years ago, I received from Governor Ellis of Georgia another ſpecies of this tree, which was ſent him by Mr. De Brahme, from Auguſta in Georgia, three hundred miles up the river Savannah.

The fruit of this kind has two wings, as deſcribed in the Plate, at B. [*Vide Tab.* XXII.]

The characters of the Halesia are as follows.

CAL. (*a*) *Perianthium* monophyllum, superum, minimum, persistens, quadridentatum: denticulis acutiusculis, erectis.

COR. (*b*) *Petalum* campanulatum, magnum, ventricosum; *limbus* quadrilobus: lobis minimis, obtusis, patulis.

STAM. (*c d*) *Filamenta* (*d*) duodecim, (raro sedecim) tubo corollæ inserta, basi connata, subulata, erecta, corollâ paulo breviora. *Antheræ* (*c*) oblongæ, obtusæ, erectæ.

PIST. (*e f g*) *Germen* (*e*) oblongum, inferum. *Stylus* (*f*) filiformis, corollâ longior, persistens. *Stigma* (*g*) simplex.

PER. (*h*) *Nux* corticata, sub cortice sulcata, oblonga, utrinque angustata, membranis angulata, quadrilocularis (*k*).

SEM. (*i*) solitaria, lineari-oblonga.

The species are,

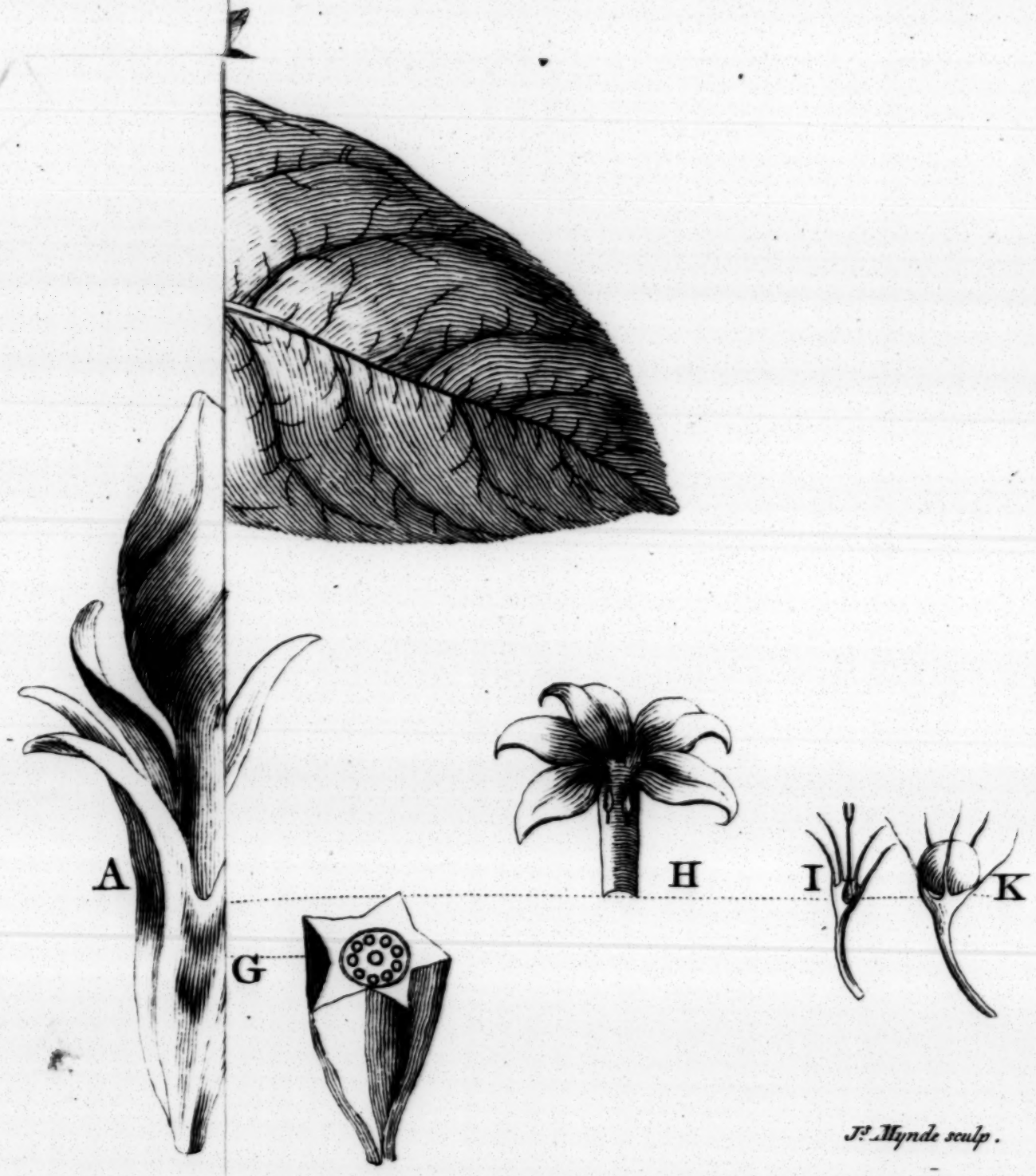
tetraptera. 1. HALESIA fructibus membranaceo-quadrangulatis.

diptera. 2. HALESIA fructibus alatis.

The other plant, which I am to describe, is known by the name of the Cape Jasmine, and is the most rare and beautiful shrub, that has yet been introduced into the European gardens, as well for the refreshing aromatic smell of its double milk-white flowers, as the perpetual verdure of its leaves, which are like those of the lemon-tree. [*Vide Tab.* XXIII.]

It

Philos **ARDENIA** *with a double Flower.*



Js. Mynde sculp.

A. *The Caly* *The Corolla of a Single flower according to D.r Linnæus.*
C. *The Tube* *the upper part of the Germen at* F. ——
G. *The Perice* *at* F.—H. *The Corolla of the common Jasmine*
cut open to *of the Flower.* K. *The fruit of the Jasmine*
above the r

GARDENIA *with a double Flower.*



A. *The Calyx of the* GARDENIA *with the flower not yet expanded to shew the contortion of the segments of the Petal.* B. *The Corolla of a single flower according to Dr. Linnæus.* C. *The Tube of the Corolla cut open to shew the 5 Stamina.* D. *The Pistillum.* E. *The top of the Calyx cut off to shew the upper part of the Germen at* F. — G. *The Pericarpium cut horizontally to shew the Rudiments of the Seeds below the receptacle of the Flower at* F. — H. *The Corolla of the common Jasmine cut open to shew the 2 Stamina.* I. *The Pistillum of the Jasmine with its Germen above the receptacle of the Flower.* K. *The fruit of the Jasmine above the receptacle of the flower.* —

It promiſes, from the thickneſs and woodineſs of its ſtem, together with its free manner of growing, to become a ſhrub of ſix or ſeven feet high.

It bears but one flower at the end of a branch; and the leaves grow oppoſite to each other on the branches.

We are indebted to Capt. Hutchinſon, of the Godolphin Indiaman, for this curious diſcovery, who, about ſix years ago, found it growing near the cape of Good Hope, and, on his arrival here, preſented it to Richard Warner, Eſq; of Woodford in Eſſex; who finding great difficulty in propagating this valuable plant, either from cuttings or by inarching it on the yellow Indian jaſmine, as he had been adviſed, I recommended him to try Mr. James Gordon, gardener at Mile-end; and, at the ſame time (Auguſt 1757), by the intereſt of my worthy friend Guſtavus Brander, Eſq; F. R. S. I procured two cuttings of it for Mr. Gordon. Theſe, with two more, which he afterwards received, he increaſed to ſo conſiderable a number, that, in order to diſpoſe of them, he advertiſed them for ſale, at five guineas a plant; and has had ſuch ſucceſs in the ſale, that, reckoning the value of the plants on hands (with a proper allowance for the falling of the price, as they become more plenty), he computes this plant will be worth at leaſt five hundred pounds ſterling to him. I mention this; becauſe I am perſuaded it muſt give you, and all lovers of vegetation, great pleaſure to ſee a man thus amply and deſervedly rewarded for his ſuperior merit in this art.

Having diſſected many dried as well as freſh ſpecimens of this rare plant, I found ſufficient evidence

(notwithſtanding the flowers being double) to prove, that it belonged to quite another claſs of plants, as different from the Jaſmine as the Roſe is from the Peony: that the fruit was below the receptacle, inſtead of being above it. But, in order to be more certain, you may remember, in July 1758, I procured a ſpecimen from Mr. Warner, for my friend Dr. Linnæus's opinion. At the ſame time I wrote to the profeſſor, that if he found it to be a new genus, agreeable to the deſcription I had ſent him, that he would pleaſe to call it Warneria, after its worthy poſſeſſor. In his anſwer, he ſets forth the impoſſibility of his being exact in determining a new genus from a double flower, agreeable to the rules he has already laid down in his Fundamenta Botanica. But theſe objections were ſoon after fortunately removed, by accidentally finding, among his dried oriental plants, a ſpecimen of the ſame kind with a ſingle flower, which, upon expanding it in warm water, and diſſecting it, he found it to agree very nearly with the deſcription I had ſent him. But Mr. Warner refuſing to have it ſo called, and chuſing that it ſhould ſtill remain a Jaſmine, as it is commonly called, I have thought no man more worthy, as a botaniſt, than our friend Dr. Garden: accordingly, the profeſſor has agreed to adopt this new genus by the name of Gardenia, which he ſays belongs to the natural order of contorted flowers, that is, to thoſe monopetalous flowers, whoſe lobes, or ſections of the limb of their petals, turn all to the right hand; ſuch as the Nerium, Plumeria, Cerbera, Cameraria, Vinca, &c. and that it ſhould be placed next to the Cerbera.

Mr.

Mr. George Dionyſius Ehret, F. R. S. has lately publiſhed a moſt elegant plate of this plant, by the name of, Jaſminum? ramo uniflore pleno, petalis coriaceis, with a note of interrogation, as a quære, before the word Jaſminum; leaving the determination, whether it is a Jaſmine or not, to a future inquiry.

The characters of the Gardenia are,

[*Vide Tab.* XXIII.]

CAL. *Perianthium* monophyllum, quinquangulare, quinquepartitum: *laciniis* enſiformibus, verticalibus, ſtrictis, erectiuſculis, perſiſtentibus.

COR. *Petalum* hypocrateriforme; *tubus* ſubcylindricus, calyce longior; *limbus* quinquepartitus, planus: *laciniis* obovatis, longitudine tubi, altero margine rectiore.

STAM. *Filamenta* quinque, tubo corollæ adnata, in fauce breviſſime libera; *Antheræ* lineares ſuberectæ, longitudine dimidii limbi.

PIST. *Germen* infra receptaculum, oblongum. *Stylus* filiformis, longitudine tubi corollæ, definens extra faucem in *Stigmata* duo, magna, craſſa, obtuſiſſima.

PER. *Bacca* ſicca, oblonga, bilocularis.

SEM. plurima, minima.

There is but one ſpecies known.

Jaſminoides. 1. GARDENIA.

I am, Dear Sir,

Your moſt affectionate

humble ſervant,

London, Nov. 20, 1760. John Ellis.

LXXXIII. *An Account of an Eclipſe of the Moon,* 22*d* November 1760, *obſerved in* Surrey-Street *in the* Strand, London; *by* James Short, *M. A. F. R. S.*

Read Dec. 11, 1760.

	h	′	″
PEnumbra very ſenſible at	7	26	0
Beginning of the eclipſe at	7	39	0
Quantity of obſcuration = 17′ 36″ at - -	8	49	0
Moon's diameter almoſt parallel to the horizon = 3′ 51″ at - - - - - - -	8	53	0
End of the eclipſe at - - - - - - -	10	8	0

LXXXIV. *An Account of the Caſe of a young Man, who had loſt the Uſe of his Hands by cleanſing Braſs Wire; by Mr.* Samuel More, *Apothecary in* Jermyn-Street. *Communicated by* Charles Morton, *M. D. Secretary to the Royal Society.*

Read Dec. 11, 1760.

THE diſorders, to which mechanics are unavoidably ſubjected by their employments, have exerciſed the pens of ſeveral ingenious writers; among whom I would not preſume to appear, but that, I hope, the publiſhing the following caſe may be of ſome ſervice: for I am certain, every one, who has been in the leaſt converſant with the labouring people of this town, muſt frequently have obſerved their hands in a condition ſomething

ſomething reſembling, though perhaps not quite ſo bad as, that of the young man, whoſe diſorder is the ſubject of the following paper.

And as there is great reaſon to believe, that the complaint, herein treated of, frequently happens to perſons employed in the dyeing buſineſs, they will hereafter have a method of cure laid before them, which, with little expence, will probably be of great ſervice to them. And, on that conſideration only, I have been induced to ſubmit this account of the diſeaſe to the inſpection of the public.

The Caſe of Francis Newman, *Nephew and Apprentice to Mr.* Newman, *Dyer, in the* Maize, Southwark, *who had loſt the Uſe of his Hands by cleanſing Braſs Wire.*

FOR the more readily explaining the following caſe, it is proper to premiſe, that, in drawing braſs wire for the pin-makers, the frequent paſſing it through the fire, to anneal it, covers it with a cruſt, which it is neceſſary to take off, before they can make uſe of it; and, for this purpoſe, it is ſent to the dyers, who, letting it lie for ſome time in the liquor, with which they have dyed what they call Saxon colours, (which liquor is compoſed of water, oil of vitriol, alum, tartar, *&c.*) and then throwing it forcibly three or four times againſt the ground, the cruſt is, by degrees, broken off, and the wire rendered bright, and fit for uſe. The gratuity given for this is generally allowed to the apprentices; and in this work Francis Newman had frequently (at his leiſure hours) employed himſelf, till about the month of Auguſt 1759, when the cuticle on the palms of his

his hands, and the inſide of his fingers, was become ſo hard and rigid, that he was no longer capable of doing either this, or any other buſineſs.

For relief of this diſorder, he applied to the perſon, who attends the family in capacity of apothecary, who gave him ſeveral doſes of purging phyſic, but without ſucceſs.

He was next admitted an out-patient at St. Thomas's hoſpital, where he attended ſix weeks or two months, but without receiving any benefit.

Somebody then told him, his complaint was owing to the ſcurvy, (to which he had been ſubject) and he accordingly applied himſelf to ſeveral perſons, who advertiſe remedies for curing that diſtemper, and, among the reſt, to Mr. Ward, of whom he had ſome pills; and once, by miſtake, took two of them for a doſe, which operated ſo violently, that every body in the family imagined he could not ſurvive it: however, he ſtill continued in the ſame condition. And now thinking, that if he was admitted an in-patient at the hoſpital, he ſhould be more likely to obtain a cure, he got himſelf admitted, and was there about two months longer; at the end of which time he was diſcharged, but in no better condition than before.

About a fortnight after this, and a twelvemonth from the beginning of his diſorder, *viz.* Auguſt 10, 1760, the perſon, who is foreman to Mr. Newman, deſired leave to write to me, for my opinion of the caſe; which being very readily granted, he deſired me, by letter, to come and ſee a young man, who, as he expreſſed it "had poiſoned his hands with braſs and oil of vitriol."

When

When I firſt viſited him, I found him with his hands quite ſtiff, and utterly incapable of any buſineſs whatever; and having already had ſo much advice, and taken ſo many medicines, he concluded his diſorder was incurable, and that he ſhould entirely loſe the uſe of his hands, the *ſkin on the palms* of them (the right hand rather the worſt of the two) having the *exact appearance of parchment*, *full of chaps*; and when I endeavoured, by force, to ſtreighten the fingers, the *blood ſtarted from every joint* of them.

After hearing the beſt account I could get of the cauſe of his complaint, I imagined, that, as the diſeaſe had been contracted by his frequently dipping his hands into a violently-acid liquor, the moſt probable method of relieving him would be, by the application of an emollient liniment, mixed with an alkaline lixivium. For this purpoſe, I ordered as follows:

℞ Ol. Olivar. ℥iv.
Lixivii. Salis Alkalin. fix. ℥ii. M.
F. Linimentum.

With this he was ordered to anoint his hands frequently, eſpecially going to bed; and, to prevent the liniment being too ſoon rubbed off, conſtantly to wear a pair of gloves.

About four days after, I found the ſkin a little ſoftened, and I could extend the fingers with leſs pain than before; and no blood iſſued, upon my endeavouring to move them. This would have encouraged me to have continued the uſe of the ſame liniment; but as he complained much of its making his hands

ſmart every time he uſed it, (and, indeed, this was the firſt application among the many he had tried, that ever gave him any uneaſineſs) I concluded, that the addition of ſome yolk of egg might leſſen the acrimony of the alkaline ſalt, without at all abating the efficacy of the liniment: I therefore compoſed the liniment thus:

℞ Ol. Olivar. ℥iv.
Lixivii. Salis Alk. fix. ℥ii.
Vitel. Ovor. N°. ii. f. Linimentum;

to be uſed as before. This mixture not giving him ſo much pain as the former, he had uſed it all in three days; and then, coming to me for more, I found his hands ſtill continue to mend; the ſkin that had grown hard ſcaling off, and a new flexible one appearing underneath; the chaps were many of them healed; and he began to have ſome uſe of his fingers. Encouraged by this ſucceſs, he continued the uſe of the laſt preſcribed liniment; and as, from his not having had the proper uſe of his fingers for ſo long a time, the joints of them had, in a great degree, loſt their motion, I adviſed him, alternately to clench his fiſt, and to ſtretch out his fingers, many times a day.

The diſorder had been ſo long upon him, and (if I may be allowed the expreſſion) had taken ſo deep root, that, although he began very ſenſibly to amend, from the firſt application of the liniment, yet it was full two months before I thought it adviſeable to leave off the uſe of it; and then, to prevent a relapſe, I gave him the following ointment:

℞ Axung. Porcin. ℥ii.
Vitel. Ovi.
Ol. Lavend. gt. v. f. Unguentum.

with

with orders to anoint his hands with it every night going to bed. This ointment he has continued to uſe about a month; and is now perfectly reſtored to the uſe of his hands, and begins again to work at his buſineſs.

During this courſe of anointing, he took no internal medicines, except three doſes of purging phyſic.

LXXXV. *A further Account of ſome Experiments made on the* Bovey *Coal* *.

Read Jan. 8, 1761. SALT of hartſhorn mixed with the phlegm that diſtilled firſt from the Bovey coal, produced no ebullition, nor air bubbles; but when mixed with the watry liquor, which aroſe with the thick oil in the latter part of the proceſs, after it had ſtood ſome weeks in a glaſs bottle, cloſe ſtopt, and was become perfectly fine, cauſed a very conſiderable ebullition, and the mixture immediately grew foul and red. In ſome days after, it grew much thicker, and had the colour of tar. The ſurface of it was covered with a bituminous pellicle, as were the ſides and bottom of the glaſs. Eighteen grains of ſalt of hartſhorn were not more than ſufficient to ſaturate the acid ſalts contained in an ounce of the liquor, which was but very little ſour to the taſte.

Spirit of nitre dropped into this bituminous liquor, ſoon after it was diſtilled, and before it had depoſited the oily particles (which rendered it cloudy), changed

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* See before, p. 534.

its colour to a deep brown; but had not that effect after the liquor was become tranſparent.

The black gritty powder, which remained after the former proceſs, was put into a coated retort, and diſtilled by a naked fire; ſo that the whole body of the retort continued red-hot for more than two hours. This brought over to the receiver near an ounce of a watry bituminous liquor, rather ſtronger than that which diſtilled with a ſand heat, and a few drops only of a thick bituminous matter, which ſtuck to that part of the receiver on which they fell. The neck of the retort was thinly incruſtated with ſomething that reſembled a ſaline concrete; but was found to be only bituminous matter. In the bottom of the retort there remained a very black gritty powder.

Of this powder, one ounce was put into a crucible, ſet in a melting furnace, and kept in a pretty ſtrong fire for an hour. The powder, after it was grown cold, appeared on the ſurface to be of a pale reddiſh colour; but was not in the leaſt altered underneath. It loſt, however, in weight, near three drachms. Some of the black powder taken out of the crucible, and thrown upon a red-hot iron, burnt without flame; but emitted plentifully a heavy black ſmoke.

Two ounces of the black powder, which had been twice diſtilled, were ſet on a clear fire, in an iron ladle, and continually ſtirred from the time that the ladle grew red-hot, and the matter began to emit a heavy black ſtinking ſmoke, till no more ſmoke aroſe from it. The calcined matter remaining in the ladle weighed two ſcruples, and ſeemed to be a kind of bole earth. This earth was evaporated in two ounces of rain water to one ounce, which, ſome days after, was poured off clean from the ſediment. This water had

had not the leaſt ſaline taſte, nor did it give any ſign of effervescence, when ſpirit of nitre was dropped into it.

From the preceding experiments it appears, that the ſubſtance called foſſil-wood conſiſts, for the moſt part, of water, and that a conſiderable quantity of this principle is ſeparated from it by a gentle heat; which ſeems to be the reaſon, why ſuch foſſil-wood, on being expoſed to the ſun and air, or kept in a dry place, ſoon becomes full of ſuperficial cracks, reſembling a piece of timber, which, by lying long on the ground in the open air, has begun to decay: that though the foſſil-wood does not, like amber, or pit-coal, yield, by diſtillation, a light oil floating on its phlegm, and a volatile acid ſalt in a concrete form, yet that a light oil, and a volatile acid ſalt, in a conſiderable quantity, are intimately mixed with the water, which diſtils from it: that this foſſil-wood differs, in ſeveral particulars, from all wood belonging to the vegetable kingdom, which has been examined by fire after the ſame manner.

1ſt, Its powder burnt in a cloſe veſſel, and kept red-hot for a much longer time, than is ſufficient to reduce the like quantity of vegetable charcoal, emits (when ſprinkled on a red-hot iron) a thick heavy black ſmoke.

2dly, The ſame powder, burnt as before-mentioned, doth not eaſily take fire, nor burn of itſelf, nor conſume to aſhes, even when expoſed, in an open crucible, to a ſtrong fire, and kept in it ignited, and almoſt white, for a conſiderable time.

3dly, The matter left by this powder, after its phlogiſtic principle has been ſeparated from it by time and air, contains no alkaline ſalt, and appears

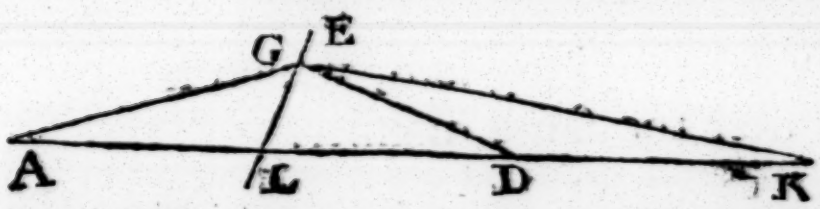
to be an aſtringent mineral earth; whereas charcoal eaſily takes fire, burns freely without ſmoke, and continues burning, till it conſumes to an aſh; which conſiſts of an alkaline ſalt, and a pure earth, fit for making cuppels; and, by theſe marks, is ſufficiently diſtinguiſhed from all mineral ſubſtances.

Groſvenor-Street, Jan. 8, 1761. Jer. Milles.

LXXXVI. *De Aberratione Luminis, in Superficiebus et Lentibus Sphæricis refractorum* *.

§ 1.

Read April 2, 1761. SI radius luminis AG, incidens in ſuperficiem quamcunque refringentem LE, inflectatur ſecundum rectam GK, et quævis recta linea AK occurrat radio incidenti AG in A, refracto GK in K; et rectæ GD, normali ad ſuperficiem refringentem LE, in D; erit rectangulum GK × DA ad rectangulum GA × DK, ut ſinus anguli incidentiæ DGA ad ſinum anguli refractionis DGK. Eſt enim DA ad GA, ut ſin. DGA ad ſin. ADG, et GK : DK :: ſin. GDK : ſin. DGK; quare, ob



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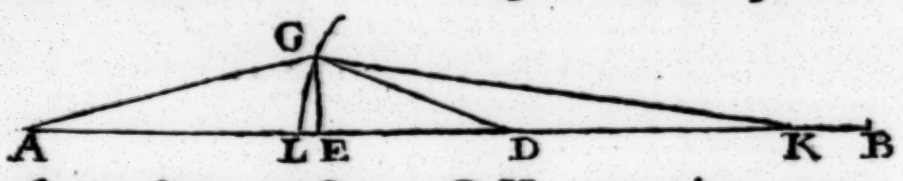
* This Paper, though ſent to England in the ſummer of the year 1760, was, by accident, prevented from being read to the Royal Society, till the 2d of April following.

ſin.

ſin. ADG = ſin. KDG, erit componendo DA × GK : GA × DK :: ſin. DGA : ſin. DGK. Quæ ratio, ſi ponatur ut i ad r, erit i. GA × DK = r. GK × DA.

§ 2.

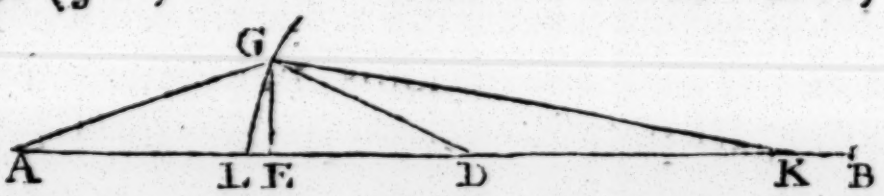
Incidat radius luminis AG in ſuperficiem ſphæricam LG, cujus centrum eſt D,



et refringatur ſecundum rectum GK, quæritur concurſus K radii refracti GK cum axe ſphæræ ALD, poſito arcu LG ſatis parvo.

A puncto incidentiæ G cadat GE normalis ad axem AD, ducaturque radius ſphæræ GD. His factis eſt per elementa, $AGq = ADq + DGq - 2AD \times DE = ADq + DLq - 2AD \times \overline{DL - LE} = \overline{AD - DL}q + 2AD \times LE = ALq + 2AD \times LE$, adeoque $AG = \sqrt{ALq + 2AD \times LE} =$ (ob LE ſatis parvam) $AL + \frac{AD \times LE}{AL}$ quamproxime. Similiter $KGq = KDq + DGq + 2KD \times DE = KDq + DLq + 2KD \times \overline{DL - LE} = \overline{KD + DL}q - 2DK \times LE = KLq - 2KD \times LE$, adeoque $KG = \sqrt{KLq - 2KD \times LE} = KL - \frac{KD \times LE}{KL}$ quamproxime.

Jam vero eſt (§ 1.) i. GA × DK = r. GK × DA, quare ſubſtitutis valoribus modo in-



ventis ipſarum GA et GK, habetur i. KD × $\left(AL + \frac{AD \times LE}{AL}\right) = r$. AD × $\left(KL - \frac{KD \times LE}{KL}\right)$, et tranſ-

tranſponendo $r.AD \times KL - i.KD \times AL = AD \times KD \times LE \times \left(\frac{i}{AL} + \frac{r}{KL}\right)$.

Ponatur jam $LA = A$, $LK = K$, radius ſphæræ $DL = a$, adeoque $AD = A + a$, et $DK = K - a$. His in noviſſima æquatione ſcriptis, erit $r K \times \overline{A+a} - i A \times \overline{K-a} = \overline{A+a} \times \overline{K-a} \times LE \times \left(\frac{i}{A} + \frac{r}{K}\right)$, unde tranſponendo et dividendo, $K = \frac{iAa}{\overline{i-r}.A - ra} - \frac{\overline{A+a} \times \overline{K-a} \times LE}{\overline{i-r}.A - ra} \times \left(\frac{i}{A} + \frac{r}{K}\right)$.

Jam ſi radius incidens AG fuerit axi AD viciniſſimus, evaneſcet LE, adeoque et terminus $\frac{\overline{A+a} \times \overline{K-a} \times LE}{\overline{i-r}.A - ra} \times \left(\frac{i}{A} + \frac{r}{K}\right)$. Quare in hoc caſu $K = \frac{iAa}{\overline{i-r}.A - ra}$, id eſt, diſtantia foci geometrici ipſi A conjugati à vertice L, erit $\frac{iAa}{\overline{i-r}.A - ra}$, unde aberratio radii refracti GK ab hoc foco erit $\frac{\overline{A+a} \times \overline{K-a} \times LE}{\overline{i-r}.A - ra} \times \left(\frac{i}{A} + \frac{r}{K}\right)$, ſumendo à foco contra directionem curſus radiorum. Quoniam vero aberratio illa ſemper eſt valde parva, erit $\frac{iAa}{\overline{i-r}.A - ra}$ valor prope verus diſtantiæ LK ſive K, adeoque in expreſſione aberrationis ſine ſenſibili errore pro K uſurpari poteſt. Sit itaque diſtantia LB foci geometrici B à vertice ſuperficiei L, ſive $\frac{iAa}{\overline{i-r}.A - ra} = B$,

eritque

eritque K ſive $LB = B - \frac{\overline{A+a}.\overline{B-a}.LE}{\overline{i-r}.A-ra} \times \left(\frac{i}{A} + \frac{r}{K}\right)$ quamproxime.

Dimidia latitudo ſuperficiei refringentis, ſive diſtantia puncti incidentiæ G ab axe, id eſt L G, dicatur L, erit quamproxime $LE = \frac{L^2}{2a}$: quare in formula modo inventa pro L E ſcribendo $\frac{L^2}{2a}$, et ulterius concinnando expreſſionem aberrationis ope æquationis $B = \frac{iAa}{\overline{i-r}.A-ra}$, habetur formula, qua in ſequentibus præcipue utemur: $LK = B - \frac{r.\overline{i-r}.B^2L^2}{2i^3} \times \left(\frac{1}{a} + \frac{1}{A}\right)^2 \times \left(\frac{r}{a} + \frac{i+r}{A}\right)$.

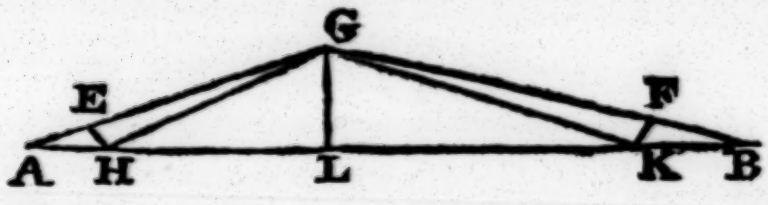
§ 3.

In formulis jam inventis ſigna ſymbolorum illi tantum caſui ſunt accommodata, quem figura exprimit, ubi radius luminis ab axe divergens ponitur incidere in ſuperficiem convexam, et poſt refractionem ad axem convergere. Accommodantur vero ad reliquos problematis caſus, mutando ſignum radii ſphæræ *a*, ſi radius luminis incidat in ſuperficiem concavam, et ſignum ipſius A, ſi radius incidens convergat ad axem. Hoc facto, ſi valor diſtantiæ L B ſive B prodit poſitivus, ſumenda eſt diſtantia illa à vertice L ſecundum directionem curſus radiorum; ſi negativus, contra eandem directionem. Aberratio vero radii à foco B, ſive B K per formulas computata, ſi fuerit poſitiva, ſumenda eſt à foco B contra directionem curſus radiorum; ſi negativa, ſecundum hanc directionem. Si radii incidentes fuerint axi paralleli,

ralleli, facienda eſt A infinita, et ſi B prodit infinita, radii refracti erunt axi paralleli, abſtrahendo ab illorum aberratione.

§ 4.

Si duo radii luminis AG, HG, in angulo quam minimo AGH inter ſe inclinati incidant fere perpendiculariter in idem punctum G ſuperficiei cujuſcunque refringentis LG, et refringantur, prior in GK, poſterior vero in GB, occurrentes lineæ rectæ cuicunque in punctis A, H, K, B; dico eſſe BK ad AH ut eſt $r.LBq$ ad $i.LAq$, poſita i ad r ut ſinus incidentiæ ad ſinum refractionis.

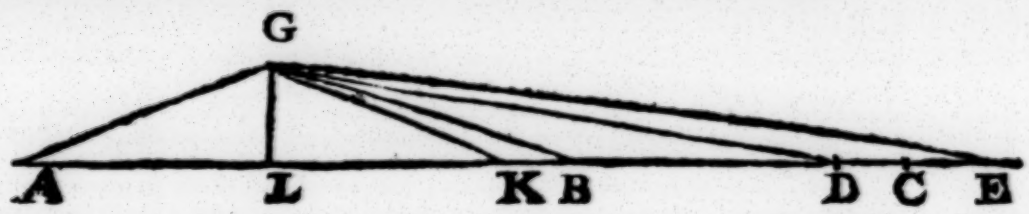


Quoniam enim anguli valde parvi ſunt ſinibus ſuis quamproxime proportionales, erunt in parvis refractionibus anguli incidentiæ et refractionis eorumque adeo differentiæ ſinibus incidentiæ et refractionis proportionales quamproxime. Eſt itaque angulus AGH, utpote differentia angulorum incidentiæ radiorum AG, HG, ad angulum BGK, ſive differentiam angulorum refractionis radiorum GK, GB, ut i ad r. Quare centro G inter hos angulos deſcriptis arcubus HE, KF, erit HD ad KF, ut $i.GH$ ad $r.GK$; ſed eſt $AH = \frac{GH.HE}{GL}$, et $BK = \frac{GK.KF}{GL}$, quare pro HE et KF, ſcribendo earum proportionales $i \times GH$ et $r \times GK$, habetur AH ad BK ut $i \times GHq$ ad $r \times GKq$, quamproxime, id eſt, ut $i \times LAq$ ad $r \times LBq$.

§ 5.

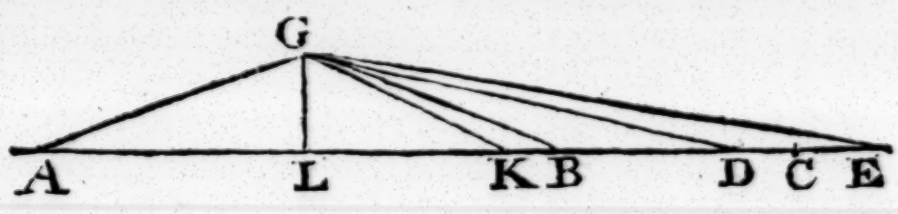
§ 5.

Data forma lentis refringentis, et data in ejus axe diſtantia foci radiorum incidentium, invenire punctum concurſus radii cujuſvis refracti cum axe.



Sit locus lentis in L, ejus axis ALBC, dimidia latitudo LG, ſive diſtantia incidentiæ ab axe, = L; radius convexitatis ſuperficiei primæ, in quam radii incidunt, = *a*; radius convexitatis ſuperficiei ſecundæ, è qua emergunt, = *b*, radius luminis quivis incidens AG, refractus GK. In axe lentis ABC ſit diſtantia foci radiorum incidentium, ſive LA = A, diſtantia foci geometrici ipſi A conjugati, ſive LB = B; diſtantia foci geometrici C ſuperficiei primæ ipſi A conjugati, ſive LC = C, et evidens eſt, eſſe etiam idem punctum C focum geometricum ſuperficiei ſecundæ conjugatum ipſi B. Sit tandem diſtantia puncti concurſus K radii refracti GK cum axe, ſive LK = K.

Radius AG in prima ſuperficie refractus tendat verſus punctum D, et concipiatur à puncto B, quod eſt focus lentis ipſi A conjugatis, radius BG incidere



in ſecundam ſuperficiem, et in ea ita refringi, ut divergat à puncto axeos E. Et dabuntur per § 2. aberrationes CE et CD, adeoque earum ſumma DE. Sed per § 4. eſt DE ad BK ut *r*. LC*q* ad *i*. LB*q*,

quare $BK = \frac{i.LBq}{r.LCq} \times DE = \frac{iB^2}{rC^2} \times DE$. Jam per § 2. eſt $CD = \frac{r.\overline{i-r}.C^2L^2}{2i^3} \times \left(\frac{1}{a}+\frac{1}{A}\right) \times \left(\frac{r}{a}+\frac{i+r}{A}\right)$ et $CE = \frac{r.\overline{i-r}.C^2L^2}{2i^3} \times \left(\frac{1}{b}+\frac{1}{B}\right) \times \left(\frac{r}{b}+\frac{i+r}{B}\right)$, quarum ſumma DE, ducta in $\frac{i.B^2}{r.C^2}$, dat aberrationem quæſitam $BK = \frac{iB^2}{rC^2} \times DE = \frac{\overline{i-r}.B^2L^2}{2i^2}\left[\left(\frac{1}{a}+\frac{1}{A}\right)^2 \times \left(\frac{r}{a}+\frac{\overline{i+r}}{A}\right)+\left(\frac{1}{b}+\frac{1}{B}\right)^2 \times \left(\frac{r}{b}+\frac{i+r}{B}\right)\right]$, quæ poſt evolutionem terminorum ſubtracta de LB, ſive B, relinquit diſtantiam puncti concurſus $LK = B - \frac{\overline{i-r}.B^2L^2}{2i^2} \times \left\{\begin{array}{l} \frac{r}{a^3}+\frac{i+3r}{Aa^2}+\frac{2i+3r}{A^2a}+\frac{i+r}{A^3} \\ +\frac{r}{b^3}+\frac{i+3r}{Bb^2}+\frac{2i+3r}{B^2b}+\frac{i+r}{B^3}\end{array}\right\}$.

Porro quoniam punctum C eſt focus ipſi A conjugatus reſpectu faciei primæ, erit $LC = \frac{iAa}{\overline{i-r}.A-ra}$; et quoniam idem punctum C eſt focus ipſi B conjugatus reſpectu ſuperficiei ſecundæ, erit $LC = -\frac{iBb}{\overline{i-r}.B-rb}$, per § 2. Hinc $\frac{iAa}{\overline{i-r}.A-ra} = -\frac{iBb}{\overline{i-r}.B-rb}$, adeoque $B = \frac{rAab}{\overline{i-r}.A.\overline{a+b}-rab}$, ſive $\frac{1}{A}+\frac{1}{B} = \frac{i-r}{r} \times \overline{\frac{1}{a}+\frac{1}{b}}$. Dato ſic valore ipſius B, cognoſcitur diſtantia concurſus radii refracti GK cum axe ſive LK, ejuſque aberratio à foco B ſive BK, quæ ob ſignum — ſumenda eſt à foco B contra directionem

directionem curſus radiorum, ſi valorem habet poſitivum; ſecundum vero, ſi negativum.

Accommodantur vero hæ formulæ ad reliquos problematis caſus eadem ratione ac dictum eſt § 3. de aberratione ſuperficiei ſimplicis.

§ 6.

Expreſſio aberrationis jam inventa, ad formam reducitur uſui magis accommodatam, conjungendo terminos, in quibus a et b eaſdem habent dimenſiones, et reducendo ſingulas partes ope æquationis $\frac{i-r}{r} \times \frac{1}{a} + \frac{1}{b} = \frac{1}{A} + \frac{1}{B} = \frac{1}{P}$, ubi P eſt diſtantia foci principalis. Hac ratione calculum ſubducendo habetur: $\frac{1}{a^3} + \frac{1}{b^3} = \frac{r}{\overline{i-r}.P} \times \left(\frac{r^2}{\overline{i-r}^2.P^2} - \frac{3r}{\overline{i-r}.P.\overline{a+b}} \right)$; $\frac{1}{A\,a^2} + \frac{1}{B\,b^2} = \frac{r}{\overline{i-r}.P} \times \left(\frac{1}{A\,a} + \frac{1}{B\,b} - \frac{1}{P.\overline{a+b}} \right)$; $\frac{1}{A^2 a} + \frac{1}{B^2 b} = \frac{r}{\overline{i-r}.P} \times \left(\frac{i-r}{r} \times \frac{1}{A\,a} + \frac{1}{B\,b} - \frac{1}{P.\overline{A+B}} \right)$; $\frac{1}{A^3} + \frac{1}{B^3} = \frac{r}{\overline{i-r}.P} \times \left(\frac{i-r}{r\,P^2} - \frac{3.\overline{i-r}}{r\,P.\overline{A+B}} \right)$.

Singulæ harum partium ductæ in ſuos reſpective coëfficientes, r, $i+3r$, $2i+r$, $i+r$, conjungantur cum ſignis propriis, et habebitur: $LK = LB - BK = B - \frac{B^2 L^2}{2\,i\,P^2} \left(\frac{i^3 - 2\,i^2 r + 2\,r^3}{\overline{i-r}^2.P} - \frac{3i+2r}{A+B} + \frac{2.\overline{i+r}}{A+B} \times \frac{A}{b} + \frac{B}{a} - \frac{r.\overline{i+2r}}{\overline{i-r}.\overline{a+b}} \right)$, vel etiam $= B - \frac{B^2 L^2}{2\,i\,P^2} \times \left(\frac{i^3}{\overline{i-r}^2.P} - \frac{3i+2r}{A+B} - \frac{2.\overline{i+r}}{A+B} \times \frac{A}{a} + \frac{B}{b} - \frac{r.\overline{i+2r}}{\overline{i-r}.\overline{a+b}} \right)$.

§ 7.

§ 7.

Invenire lentem, quæ radios luminis à data diſtantia advenientis in alia data diſtantia colligat cum minima aberratione, ipſamque aberrationem minimam.

Retentis omnibus ſymbolis ut ſupra, ſumantur differentialia aberrationis modo inventæ, $\frac{B^2L^2}{2i.P^2} \times \left(\frac{i^3}{\overline{i-r}^2.P} - \frac{3i+2r}{A+B} - \frac{2.\overline{i+r}}{A+B} \times \frac{\overline{A}}{a} + \frac{\overline{B}}{b} - \frac{r.\overline{i+2r}}{\overline{i-r}.\overline{a+b}}\right)$, poſitis a et b variabilibus, et habebitur $\frac{2.\overline{i+r}}{A+B} \times \frac{\overline{A\,da}}{a^2} \times \frac{\overline{B\,db}}{b^2} + \frac{r.\overline{i+2r}}{i-r} \times \frac{da+db}{\overline{a+b}^2} = 0$; ſed ob $\frac{1}{a} + \frac{1}{b} = \frac{r}{\overline{i-r}.P}$, eſt $\frac{da}{a^2} + \frac{db}{b^2} = 0$; quare tollendo per comparationem harum æquationum differentialia, invenitur: $\frac{b-a}{b+a} = \frac{2.\overline{i+r}.\overline{i-r}}{r.\overline{i+2r}} \times \frac{A-B}{A+B}$, adeoque $\frac{a}{b} = \frac{\overline{4r^2+ri-2i^2}.A + i.\overline{2i+r}.B}{\overline{4r^2+ri-2i^2}.B + i.\overline{2i+r}.A}$; unde per æquationem $\frac{1}{a} + \frac{1}{b} = \frac{r}{i-r} \times \frac{\overline{1}}{A} + \frac{1}{B}$, colliguntur radii facierum lentis quæſitæ, ſcil. $a = \frac{2.\overline{i-r}.\overline{i+2r}.AB}{\overline{i.2i+r}.A + \overline{4r^2+ri-2i^2}.B}$, et $b = \frac{2.\overline{i-r}.\overline{i+2r}.AB}{i.\overline{2i+r}.B + \overline{4r^2+ri-2i^2}.A}$; ſive $\frac{1}{a} = \frac{i.\overline{2i+r}}{2.\overline{i-r}.\overline{i+2r}} \times \frac{1}{B} + \frac{4r^2+ri-2i^2}{2.\overline{i-r}.\overline{i-2r}} \times \frac{1}{A}$; et $\frac{1}{b} = \frac{i.\overline{2i+r}}{2.\overline{i-r}.\overline{i+2r}} \times \frac{1}{A} + \frac{4r^2+ri-2i^2}{2.\overline{i-r}.\overline{i+2r}} \times \frac{1}{B}$.

Hic

Hic valores radiorum *a* et *b* ſubſtituti in formula aberrationis generali § 6. dant ipſam aberrationem minimam $= \frac{i \cdot B^2 L^2}{2 \cdot \overline{i+2r} \cdot P^3} \times \left(\frac{P}{A+B} + \frac{r \cdot \overline{4i-r}}{4 \cdot \overline{i-r}^2} \right)$.

§ 8.

Poſtquam in § præcedente formam lentis invenimus, quæ in datis circumſtantiis aberrationem minimam producat, et ſimul ipſam ejus aberrationem; rationi conſentaneum eſt, inveſtigationem aberrationis lentium ab hac forma diſcendentium ita aggredi, ut ſimul appareat relatio formarum harum lentium ad illius formam, et aberrationis harum ad illius aberrationem. Solent enim concluſiones prodire concinniores, ſi ad caſum inter reliquos maxime ſingularem et unicum referantur, qualis hic eſt caſus aberrationis minimæ.

Scribatur brevitatis cauſa *b* pro $\frac{i \cdot \overline{2i+r}}{\overline{2 \cdot i-r} \cdot \overline{i+2r}}$, et K pro $\frac{4r^2+ri-2i^2}{\overline{2 \cdot i-r} \cdot \overline{i+2r}}$, et erit pro determinandis radiis *a* et *b* lentis aberrationis minimæ, $\frac{1}{a} = \frac{b}{B} + \frac{K}{A}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B}$, per § 7. Ponatur jam generaliter pro radiis *a* et *b* facierum cujuſvis alius lentis, $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$; quos valores ipſarum $\frac{1}{a}$ et $\frac{1}{b}$ tales aſſumo, ut earum ſumma $\frac{1}{a} + \frac{1}{b}$ ſit $= \overline{b+K} \times \overline{\frac{1}{A} + \frac{1}{B}}$, ſive $\frac{r}{i-r} \times \overline{\frac{1}{A} + \frac{1}{B}}$, quemadmodum

dum oportet. Subſtituantur hi valores ipſarum $\frac{1}{a}$ et $\frac{1}{b}$ in formula aberrationis generali, $\frac{B^2 L^2}{2 i P^2} \times \left(\frac{i^3}{P} - \frac{3i+2r}{A+B} - \frac{2 \cdot \overline{i+r}}{A+B} \times \frac{A}{a} + \frac{B}{b} - \frac{r \cdot \overline{i+2r}}{\overline{i-r} \cdot \overline{a+b}}\right)$ § 6. et peracto calculo invenietur $\frac{i B^2 L^2}{2 \cdot \overline{i+2r} \cdot P^3} \left(\frac{P}{A+B} + \frac{\overline{4i-r} \cdot r}{4 \cdot \overline{i-r}^2} \frac{\overline{i+2r}^2}{i^2} x^2\right)$ pro aberratione lentis, cujus facies lumini obverſa radium habeat a, et facies à lumine averſa radium habeat b, exiſtentibus $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$; P diſtantia foci principalis, A diſtantia foci radiorum incidentium, et B refractorum.

Commodum itaque hic accidit, ut numerus x, qui relationem formæ lentis ad lentem aberrationis minimæ deſignat, idem quoque relationem aberrationis ejus ad aberrationem minimam ratione non minus ſimplici exhibeat.

§ 9.

Si in formulis § præced. exhibitis, pro radiis facierum lentis ejuſque aberratione, ſtatuatur $x = 0$, radit lens ad eam formam, quæ aberrationem dat minimam, et aberratio ejus ad aberrationem minimam. Si vero non fuerit $x = 0$, apparet ex iiſdem formulis, cuivis aberrationis quantitati, quæ major ſit aberratione minima, duas diverſas reſpondere lentis formas, quarum utraque definitur æquationibus $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{a} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$, ubi x pro altera

altera positive, altera negative accipitur. Nam in formula aberrationis $\frac{i B^2 L^2}{2 . \overline{i+2r} . P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r} . r}{4 . \overline{i-r}^2} + \frac{\overline{i+2r}^2}{i^2} x^2 \right)$ non comparet nisi quadratum ipsius x, quod idem manet, sive radix x sumatur positive, sive negative.

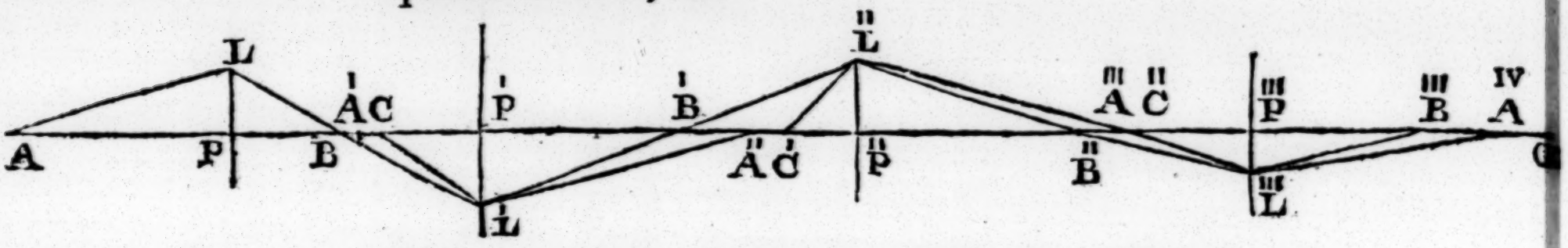
Quoniam aberratio quævis $\frac{i B^2 L^2}{2 . \overline{i+2r} . P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r} . r}{4 . \overline{i-r}^2} + \frac{\overline{i+2r}^2}{i^2} x^2 \right)$ est ad aberrationem minimam $\frac{i B^2 L^2}{2 . \overline{i+2r} . P^3} \times \left(\frac{P}{A+B} + \frac{\overline{4i-r} . r}{4 . \overline{i-r}^2} \right)$ in ratione majoris inæquabilitatis, statuatur illa ratio $1 + m^2$ ad 1; assumendo m pro numero quovis, et invenitur $x = \pm \frac{m i}{i+2r} \sqrt{\left(\frac{P}{A+B} + \frac{\overline{4i-r} . r}{4 . \overline{i-r}^2} \right)}$. Et sic ex data ratione aberrationis alicujus lentis ad aberrationem minimam, datur illi correspondens numerus x, adeoque et radii facierum ejus, per superiora.

Numerus hic x, quoniam simul et formam et aberrationem suæ lentis indicat, dicatur *index* lentis, et scribatur brevitatis causa f pro $\frac{\overline{4i-r} . r}{4 . \overline{i-r}^2}$, et g pro $\frac{1+2r}{i}$, ut aberratio sit $\frac{B^2 L^2}{2g . P^3} \times \left(\frac{P}{A+B} + f + g^2 x^2 \right)$.

§ 10.

In linea recta A O, tanquam axe communi, dispositæ intelligantur lentes quotcunque P, $\overset{\text{I}}{\text{P}}$, $\overset{\text{II}}{\text{P}}$, $\overset{\text{III}}{\text{P}}$, &c. quarum foci conjugati sint respective, lentis

primæ A et $\overset{\text{I}}{A}$; ſecundæ $\overset{\text{I}}{A}$ et $\overset{\text{II}}{A}$; tertiæ $\overset{\text{II}}{A}$ et $\overset{\text{III}}{A}$; quartæ $\overset{\text{III}}{A}$ et $\overset{\text{IV}}{A}$, &c.



Radius luminis, veniens à puncto A, refringatur in omnibus his lentibus in punctis L, $\overset{\text{I}}{L}$, $\overset{\text{II}}{L}$, $\overset{\text{III}}{L}$, &c. pergens ſecundum curſum A L $\overset{\text{I}}{L}$ $\overset{\text{II}}{L}$ $\overset{\text{III}}{L}$, et poſt ſingulas refractiones occurrat axi A O in punctis B, $\overset{\text{I}}{B}$, $\overset{\text{II}}{B}$, $\overset{\text{III}}{B}$, &c. aberrans à focis $\overset{\text{I}}{A}$, $\overset{\text{II}}{A}$, $\overset{\text{III}}{A}$, $\overset{\text{IV}}{A}$, &c. longitudinibus $\overset{\text{I}}{A}$ B, $\overset{\text{II}}{A}$ $\overset{\text{I}}{B}$, $\overset{\text{III}}{A}$ $\overset{\text{II}}{B}$, $\overset{\text{IV}}{A}$ $\overset{\text{III}}{B}$, &c. Quæruntur jam hæ aberrationes.

Lentium P, $\overset{\text{I}}{P}$, $\overset{\text{II}}{P}$, $\overset{\text{III}}{P}$, &c. diſtantiæ focorum principalium dicantur reſpective P, $\overset{\text{I}}{P}$, $\overset{\text{II}}{P}$, $\overset{\text{III}}{P}$, &c. et indices x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, $\overset{\text{III}}{x}$, &c. Diſtantiæ focorum conjugatorum à ſuis reſpective lentibus ſint: pro lente prima P, P A = A, P $\overset{\text{I}}{A}$ = B; pro lente ſecunda $\overset{\text{I}}{P}$, $\overset{\text{I}}{P}$ $\overset{\text{I}}{A}$ = $\overset{\text{I}}{A}$, $\overset{\text{I}}{P}$ $\overset{\text{II}}{A}$ = $\overset{\text{I}}{B}$; pro lente tertia $\overset{\text{II}}{P}$, $\overset{\text{II}}{P}$ $\overset{\text{II}}{A}$ = $\overset{\text{II}}{A}$, $\overset{\text{II}}{P}$ $\overset{\text{III}}{A}$ = $\overset{\text{II}}{B}$; pro lente quarta $\overset{\text{III}}{P}$, $\overset{\text{III}}{P}$ A = A, et $\overset{\text{III}}{P}$ $\overset{\text{IV}}{A}$ = B, et ita porro: ponatur præterea P L, ſive diſtantia puncti incidentiæ in primam lentem ab axe = L, et erit $\overset{\text{I}}{P}$ $\overset{\text{I}}{L}$ = $\frac{\overset{\text{I}}{A}}{B}$ × L, $\overset{\text{II}}{P}$ $\overset{\text{II}}{L}$ = $\frac{\overset{\text{I}}{A} . \overset{\text{II}}{A}}{B . \overset{\text{I}}{B}}$ × L, $\overset{\text{III}}{P}$ $\overset{\text{III}}{L}$ = $\frac{\overset{\text{I}}{A} . \overset{\text{II}}{A} . \overset{\text{III}}{A}}{B . \overset{\text{I}}{B} . \overset{\text{II}}{B}}$ × L, et ita porro.

In

I. Vidimus in § 9. aberrationem $\overset{I}{A}B$ lentis primæ P eſſe $\frac{B^2 L^2}{2g \cdot P^3} \times \left(\frac{P}{A+B} + f + g^2 x^2\right)$, ubi $f = \frac{4i - r \cdot r}{4 \cdot i - r^2}$, et $g = \frac{i + 2r}{i}$.

II. Concipiatur à foco ſecundæ lentis $\overset{II}{A}$ ipſi $\overset{I}{A}$ conjugato in illam incidere radius $\overset{II}{A}\overset{I}{L}$, qui refractus ſecundum $\overset{I}{L}C$, conveniat cum axe in C, aberrans à foco $\overset{I}{A}$ longitudine $\overset{I}{A}C$: et erit per § 9. aberratio $\overset{I}{A}C = \frac{\overset{I}{A}^4 L^2}{2g \cdot B^2 \cdot P^3} \times \left(\frac{\overset{I}{P}}{\overset{I}{A}+\overset{I}{B}} + f + g^2 \overset{I}{x}^2\right)$, ſcribendo ſcilicet $\frac{\overset{I}{A}}{B} \times L$ pro L, $\overset{I}{B}$ pro A, $\overset{I}{A}$ pro B, et P pro P. Hæc aberratio $\overset{I}{A}C$, addita ad aberrationem primæ lentis modo inventam, $\overset{I}{A}B$, dat ſummam BC. Eſt vero BC ad $\overset{I}{B}\overset{II}{A}$ ut $\overset{I}{P}\overset{I}{A}q$ ad $\overset{I}{P}\overset{II}{A}q$, ſive $\overset{I}{A}^2$ ad $\overset{I}{B}^2$, id eſt quantitates aberrationum à focis conjugatis ſunt ut quadrata diſtantiarum focorum à lente, quod facile probari poteſt. Ergo aberratio $\overset{II}{A}\overset{I}{B}$, à refractione per duas lentes P et $\overset{I}{P}$ producta, habetur $= \frac{\overset{I}{B}^2}{\overset{I}{A}^2} \times BC$

$$= \frac{\overset{I}{B}^2 L^2}{2g} \times \left[\frac{B^2}{\overset{I}{A}^2 \cdot P^3}\left(\frac{P}{A+B} + f + g^2 x^2\right) + \frac{\overset{I}{A}^2}{B^2 \cdot \overset{I}{P}^3}\left(\frac{\overset{I}{P}}{\overset{I}{A}+\overset{I}{B}} + f + g^2 \overset{I}{x}^2\right)\right].$$

III. Concipiatur ſimiliter à foco interiori $\overset{III}{A}$ lentis tertiæ $\overset{II}{P}$ incidere in illam radium $\overset{III}{A}\overset{II}{L}$, cumque refractum convenire cum axe in puncto $\overset{I}{C}$, et aberrare à foco exteriore $\overset{II}{A}$ longitudine $\overset{II}{A}\overset{I}{C}$, quæ per § 9. eſt $\frac{\overset{I}{A}^2 . \overset{II}{A}^2 . L^2}{2g . B^2 . \overset{I}{B}^2 . \overset{II}{P}^3} \times \left(\frac{\overset{II}{P}}{\overset{II}{A} + \overset{II}{B}} + f + g^2 \overset{II}{x}^2 \right)$. Hæc addita ad aberrationem duarum priorum lentium modo inventam, $\overset{II}{A}\overset{I}{B}$, dat ſummam $\overset{II}{A}\overset{I}{B} + \overset{II}{A}\overset{I}{C} = \overset{I}{B}\overset{I}{C}$, deinde fiat ut $\overset{II}{A}^2$ ad $\overset{II}{B}^2$; ita hæc ſumma $\overset{I}{B}\overset{I}{C}$ ad aberrationem trium lentium $\overset{III}{A}\overset{II}{B} = \frac{\overset{II}{B}^2 . L^2}{2g} \times \left[\frac{B^2 . \overset{I}{B}^2}{\overset{II}{A}^2 . \overset{I}{A}^2 . P^3} \left(\frac{P}{A + B} + f + g^2 x^2 \right) + \frac{\overset{I}{B}^2 \overset{I}{A}^2}{B^2 . \overset{II}{A}^2 . \overset{I}{P}^3} \left(\frac{\overset{I}{P}}{\overset{I}{A} + \overset{I}{B}} + f + g^2 \overset{I}{x}^2 \right) + \frac{\overset{I}{A}^2 . \overset{II}{A}^2}{\overset{I}{B}^2 . B^2 . \overset{II}{P}^3} \left(\frac{\overset{II}{P}}{\overset{II}{A} + \overset{II}{B}} + f + g^2 \overset{II}{x}^2 \right) \right]$.

IV. Eadem ratione invenitur aberratio $\overset{IV}{A}\overset{III}{B}$ poſt refractionem per quatuor lentes P, $\overset{I}{P}$, $\overset{II}{P}$, $\overset{III}{P}$, $= \frac{\overset{III}{B}^2 . L^2}{2g} \times \left[\frac{B^2 . \overset{I}{B}^2 . \overset{II}{B}^2}{\overset{III}{A}^2 . \overset{II}{A}^2 . \overset{I}{A}^2 . P^3} \times \left(\frac{P}{A + B} + f + g^2 x^2 \right) + \frac{\overset{I}{B}^2 . \overset{II}{B}^2 . \overset{I}{A}^2}{B^2 . \overset{II}{A}^2 . \overset{I}{A}^2 . P^3} \times \left(\frac{\overset{I}{P}}{\overset{I}{A} + \overset{I}{B}} + f + g^2 \overset{I}{x}^2 \right) + \frac{\overset{II}{B}^2 . \overset{I}{A}^2 . \overset{II}{A}^2}{\overset{I}{B}^2 . B^2 . \overset{III}{A}^2 . \overset{II}{P}^3} \times \left(\frac{\overset{II}{P}}{\overset{II}{A} + \overset{II}{B}} + f + g^2 \overset{II}{x}^2 \right) + \frac{\overset{I}{A}^2 . \overset{II}{A}^2 . \overset{III}{A}^2}{\overset{II}{B}^2 . \overset{I}{B}^2 . B^2 . \overset{III}{P}^3} \left(\frac{\overset{III}{P}}{\overset{III}{A} + \overset{III}{B}} + f + g^2 \overset{III}{x}^2 \right) \right]$. Et eadem ratione progredi licet ad definiendam

definiendam aberrationem radiorum poſt tranſitum per plures lentes.

In his computationibus omnes lentes concipiuntur convexæ, et foci radiorum incidentium ponuntur jacere ante lentes ſuas, refractorum vero poſt lentes reſpectu curſus radiorum, quemadmodum figura exhibet. Formulæ autem allatæ ad quoſvis alios caſus facile accommodantur, mutando ſigna ſymbolorum, prout requirit mutatio hypotheſeos, quod monuiſſe ſufficiat.

§ 11.

Datis poſitionibus et focorum principalium diſtantiis lentium quotcunque, oporteat invenire formas earum, quæ efficiant, ut radii luminis, à dato in communi earum axe puncto advenientes, poſt refractionem colligantur in ultimo foco ſine abberratione, ſi fieri poteſt.

Solutio problematis generaliter abſolvitur, ponendo expreſſionem aberrationis, datæ multitudini lentium convenientem, (§ 10.) nihilo æqualem, quo ipſo habetur æquatio definiens rationem requiſitam indicum ſingularum lentium x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, $\overset{\text{III}}{x}$, &c. Et quoniam relatio omnium indicum unica hac æquatione continetur, patet multum hic locum eſſe arbitrariæ nonnullorum aſſumtioni, prout requirunt vel ſuadent aperturæ lentium aliæque circumſtantiæ ex ſcopo inſtrumenti dijudicandæ, de quibus hic non eſt ſermo. In genere tantum cavendum eſt, ne aſſumtio ita fiat, ut aliquis indicum prodeat imaginarius.

Determinatis ſic indicibus x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, $\overset{\text{III}}{x}$, &c. inveniuntur radii facierum per § 8. Dicatur ſcilicet radius faciei

faciei anterioris ſingularum lentium a, poſterioris b; diſtantia foci principalis à lente, P; diſtantia foci radiorum incidentium ante lentem, A; diſtantia foci radiorum refractorum poſt lentem, B; et index ad lentem pertinens, x; et erit $\frac{1}{a} = \frac{b}{B} + \frac{K}{A} + \frac{x}{P}$, et $\frac{1}{b} = \frac{b}{A} + \frac{K}{B} - \frac{x}{P}$, ubi $b = \frac{i \cdot \overline{2i+r}}{2 \cdot \overline{i-r} \cdot \overline{i+2r}}$, et $K = \frac{4r^2 + ri - 2i^2}{2 \cdot \overline{i-r} \cdot \overline{i+2r}}$, unde dabuntur ſingularum lentium radii a et b.

Si nullus detur valor realis indicum x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, $\overset{\text{III}}{x}$, &c. id quod accidit, ſi omnes æquationis termini ad unam partem rejecti fuerint ejuſdem ſigni, caſus propoſitus eſt impoſſibilis.

§ 12.

Exemplum I. Ex duabus lentibus teleſcopium componere, quod ſit liberum ab aberratione, et objecta viſa amplificet in data ratione n ad 1. Abſtrahitur autem hic a reliquis boni teleſcopii requiſitis.

Lentes duæ primæ P et $\overset{\text{I}}{\text{P}}$ repræſentent binas lentes teleſcopii quæſiti, P quidem objectivam et $\overset{\text{I}}{\text{P}}$ ocularem (vid. fig. § 10.). Aberratio harum duarum lentium (§ 10. caſ. 11.) nihilo æqualis poſita dat æquationem generalem: $g^2 \times \left(\frac{B^2 \cdot x^2}{\overset{\text{I}}{A^2} \cdot P^3} + \frac{\overset{\text{I}}{A^2} \cdot \overset{\text{I}}{x^2}}{B^2 \cdot \overset{\text{I}}{P^3}}\right) + f \times \left(\frac{B^2}{\overset{\text{I}}{A^2} \cdot P^3} + \frac{\overset{\text{I}}{A^2}}{B^2 \cdot \overset{\text{I}}{P^3}}\right) + \frac{B^2}{\overset{\text{I}}{A^2} \cdot P^2 \cdot \overline{A+B}} + \frac{\overset{\text{I}}{A^2}}{B^2 \cdot \overset{\text{I}}{P^2} \cdot \overline{\overset{\text{I}}{A}+\overset{\text{I}}{B}}} = 0$. Quoniam per conſtitutionem teleſcopii, radii luminis

minis in lentem objectivam incidentes, et è lente oculari emergentes, ſunt axi teleſcopii paralleli, erit A infinita, adeoque $B = P$; item $\overset{I}{B}$ infinita, adeoque $A = \overset{I}{P}$. Quare deletis duobus ultimis æquationis terminis, utpote evaneſcentibus, et in reliquis in ſcribendo P et $\overset{I}{P}$ pro B et $\overset{I}{A}$, habetur pro caſu hujus exempli æquatis $g^2 \times (P x^2 + \overset{I}{P}\overset{I}{x^2}) + f \times (P + \overset{I}{P}) = o$, relationem indicum x et $\overset{I}{x}$ definiens, requiſitam ad id, ut aberratio radiorum è lente oculari emergentium nulla ſit. Jam ſi in hac æquatione P et $\overset{I}{P}$ ponantur ejuſdem ſigni, ſive utraque lens convexa, vel utraque concava, nullus valor realis haberi poteſt pro x et $\overset{I}{x}$, adeoque problema in hoc caſu erit impoſſibile. Statuatur itaque $\overset{I}{P}$ negativa, ſive lens ocularis concava, ſumendo pro exponente amplificationis objecti teleſcopio viſi, n, numerum quemvis poſitivum, et faciendo $\overset{I}{P} = -\frac{I}{n}P$, ut patet ex opticis. Scripto hoc valore pro $\overset{I}{P}$ in æquatione indicum, evadit illa $g^2 \times (\overset{I}{x^2} - n x^2) = f \times \overline{n-1}$. Horum itaque indicum utrumlibet pro lubitu aſſumere licet, modo ne $\overset{I}{x}$ fiat minor, quam $\frac{\sqrt{\overline{n-1} \cdot f}}{g}$, quod redderet indicem x imaginarium. Altero indicum ſic aſſumto, alter determinatur per allatam æquationem $g^2 \times (\overset{I}{x^2} - n x^2) = f \cdot \overline{n-1}$. Quo facto radii facierum utriuſque lentis prodeunt per § 11. ut ſequitur:

Radius faciei anterioris lentis objectivæ $= \frac{P}{b + x}$, poſterioris $= \frac{P}{K - x}$;

Radius faciei anterioris lentis ocularis $= \frac{\overset{I}{P}}{K + \overset{I}{x}}$, poſterioris $= \frac{\overset{I}{P}}{b - \overset{I}{x}}$, ubi $\overset{I}{P}$ ſumenda eſt negativa, ſive $\overset{I}{P} = - \frac{I}{n} P$, ut dictum eſt.

Pro quovis itaque pari valorum correſpondentium indicum x et $\overset{I}{x}$, quatuor prodeunt diverſæ problematis ſolutiones, ob ſigna duplicia ipſarum x et $\overset{I}{x}$, unde duæ prodeunt formæ utriuſque lentis, quarum quælibet combinari poteſt cum binis formis alterius lentis.

Si detur vel aſſumatur forma alterutrius lentis, forma alterius per ſupradicta commode determinatur. Sit v. gr. lens objectiva plano-convexa, habens faciem convexam antrorſum verſam, planam retrorſum. Radius faciei ejus poſterioris $\frac{P}{K - x}$ ſtatuatur infinitus, et erit $x = K$, adeoque radius faciei, anterioris $\frac{P}{b + x} = \frac{P}{b + K} = \frac{i - r}{r} \times P$. Pro x ſcribatur K in æquatione indicum $\overset{I}{x}^2 - n x^2 = \frac{\overline{n - I} \cdot f}{g^2}$, et erit $\overset{I}{x} = \sqrt{\frac{\overline{n - I} \cdot f}{g^2} + n K^2}$, unde dantur radii facierum lentis ocularis, idque dupliciter, ob ſignum duplex valoris ipſius $\overset{I}{x}$.

Vidimus

Vidimus in ſolutione hujus problematis, aberrationem à duabus lentibus productam non poſſe evaneſcere, ſi utraque fuerit convexa, vel utraque concava, cujuſcumque ſint formæ et quomodocunque componantur. Idem quoque obtinet in quacunque lentium multitudine, quæ omnes ſint convexæ, vel omnes concavæ. Omnes enim lentes, quarum facies ſuperficierum ſphæricarum ſunt ſegmenta, aberrationes a focis ſuis eo producunt, quod radios luminis refractione nimium inflectunt; unde facile perſpicitur, lentem convexam, convexis additam, vel concavam concavis, errores a prioribus productos augere. Quare ut aberratio a foco ultimo evaneſcat, debebunt lentium aliæ eſſe convexæ, aliæ concavæ, quo nimiæ radiorum incurvationes in unam partem corrigantur per nimias incurvationes factas in partem contrariam.

§ 13.

Exemplum II. Propoſitum ſit inveſtigare formas binarum lentium, quæ juxta ſe poſitæ radios incidentes axi parallelos in data à lentibus diſtantia R colligant, citra aberrationem ex figura facierum ſphærica oriundam.

Quoniam in caſu hujus exempli radii incidentes ponuntur paralleli erit in æquatione (§ 10. caſ. 2.) pro relatione binorum indicum x et $\overset{\text{I}}{x}$, A infinita, adeoque $B = P$. Et quoniam lentes ponuntur juxta ſe poſitæ, erit earum diſtantia $B + \overset{\text{I}}{A} = 0$, ſive $\overset{\text{I}}{A} = -B = -P$. Præterea eſt etiam $\frac{\text{I}}{\overset{\text{I}}{B}} = \frac{\text{I}}{P} + \frac{\text{I}}{\overset{\text{I}}{P}}$, unde $\overset{\text{I}}{B} - P = -\frac{P^2}{P + \overset{\text{I}}{P}}$. Subſtitutis itaque his

valoribus, erit æquatio pro relatione indicum in caſu præſenti: $g^2 . (\overset{I}{P}{}^3 . x^2 + P^3 . \overset{I}{x}{}^2) + f . P^3 + \overset{I}{P}{}^3 = P . \overset{I}{P} . \overline{P + \overset{I}{P}}$, ſive $P^3 \overset{I}{x}{}^2 + \overset{I}{P}{}^3 x^2 = \frac{P + \overset{I}{P}}{g^2} \times (\overline{1 + f . P \overset{I}{P}} - f . P^2 + \overset{I}{P}{}^2)$, quæ aſſumto numero quovis n, et ſtatuendo $\overset{I}{P} = n P$, evadit $\overset{I}{x}{}^2 + n^3 x^2 = \frac{n + 1}{g^2} \times (n - \overline{n^2 - n + 1} . f)$. Ne autem caſus propoſitus fiat impoſſibilis, ob utramque lentem convexam, vel utramque concavam, per n ſemper intelligi debet numerus negativus. Aſſumatur jam pro Cubitu alteruter indicum x et $\overset{I}{x}$, et dabitur alter per æquationem allatam.

Determinato vero utroque indice, radii facierum erunt ut ſequitur:

Radius faciei anterioris lentis primæ $= \frac{P}{b + x}$, poſterioris $= \frac{P}{K - x}$;

Radius faciei anterioris lentis ſecundæ $= \frac{\overset{I}{P}}{\overline{n + 1} . b - n K + \overset{I}{x}}$, poſterioris $= \frac{\overset{I}{P}}{\overline{n + 1} . K - n b - \overset{I}{x}}$, ubi eſt $n P = \overset{I}{P}$, poſito n numero negativo.

Diſtantia foci principalis ſyſtematis binarum lentium P et $\overset{I}{P}$ dicatur R, et quoniam eſt $\frac{I}{R} = \frac{I}{P} + \frac{I}{\overset{I}{P}}$, erit $P = \frac{n + 1}{n} R$, et $\overset{I}{P} = \overline{n + 1} . R$, quare in expreſſionibus radiorum ſcribendo hos valores pro P et $\overset{I}{P}$, habebuntur:

Radius faciei anterioris lentis primæ $= \frac{\frac{n+1}{n}R}{h+x}$, posterioris $= \frac{\frac{n+1}{n}R}{K-x}$;

Radius faciei anterioris lentis secundæ $= \frac{\overline{n+1}.R}{\overline{n+1}.h-nK+\overset{1}{x}}$, posterioris $= \frac{\overline{n+1}.R}{\overline{n+1}.K-nh-\overset{1}{x}}$.

Et hæ lentes juxta se positæ radios incidentes axi parallelos in distantia à lente, R, colligent sine aberratione, si distantia foci lentis concavæ major fuerit distantia foci lentis convexæ; si autem contra dispergentur radii sine aberratione à puncto ante lentem, cujus distantia positive sumta est R.

Pro casu singulari hujus exempli, ponamus requiri formas binarum lentium ita comparatarum, ut distantia foci principalis lentis concavæ sit ad distantiam foci principalis lentis convexæ, ut 3 ad 2, et ut radii colligantur in distantia R à lentibus. Si lens anterior debet esse convexa, erit $n = -\frac{3}{2}$, et æquatio definiens relationem indicum lentis convexæ x et lentis concavæ $\overset{1}{x}$, $8\overset{1}{x}^2 - 27x^2 = \frac{19f+6}{g^2}$. Determinatis vero decenter indicibus x et $\overset{1}{x}$, radii facierum erunt, ut sequitur:

Radius faciei anterioris lentis primæ $= \frac{R}{3h+3x}$, posterioris $= \frac{R}{3K-3x}$;

Radius faciei anterioris lentis secundæ $= -\frac{R}{3K-h+2\overset{1}{x}}$, posterioris $= -\frac{R}{3h-K-2\overset{1}{x}}$.

Si vero lens anterior debet esse concava, erit $n = -\frac{2}{3}$, et relatio indicum $8x^2 - 27\overset{1}{x}^2 = \frac{19f+6}{g^2}$, radii vero facierum ut sequitur:

Radius faciei anterioris lentis primæ $= -\frac{R}{2b+2x}$, posterioris $= -\frac{R}{2K-2x}$;

Radius faciei anterioris lentis secundæ $= \frac{R}{b+2K+3\overset{1}{x}}$, posterioris $= \frac{R}{K+2b-3\overset{1}{x}}$.

§ 14.

Exemplum III. Propositum sit, telescopium ex tribus lentibus componere, quarum prima P et secunda $\overset{1}{P}$ juxta se positæ vitrum objectivum duplex constituant, cujus distantia foci principalis sit R; tertia vero sive ocularis $\overset{II}{P}$, habeat distantiam foci $\overset{II}{P} = \frac{R}{m}$, ut potentia amplificandi designetur per numerum m. Ponatur præterea $\overset{1}{P} = n\,P$, intelligendo per n numerum datum negativum, eumque unitate majorem, si lens convexa anterior est collocanda, minorem si posterior, ut radii post refractionem in vitro objectivo convergant, quemadmodum expositum est in exemplo præcedente. Quæruntur autem formæ harum trium lentium, ut telescopium liberum sit ab omni erratione ex figura illarum sphærica oriunda.

Aberratio à tribus lentibus P, $\overset{1}{P}$, $\overset{II}{P}$, oriunda (§ 10. cas. III.) nihilo æqualis posita, dat $\frac{B^2 \cdot \overset{1}{B}^2}{\overset{II}{A}^2 \cdot \overset{1}{A}^2 \cdot P^3}\left(\frac{P}{A+B}\right.$

+

$$+f+g^2x^2)+\frac{\overset{\text{I}}{B}^2\cdot\overset{\text{I}}{A}^2}{B^2\cdot\overset{\text{II}}{A}^2\cdot\overset{\text{I}}{P}^3}\left(\frac{\overset{\text{I}}{P}}{\overset{\text{I}}{A}+\overset{\text{I}}{B}}+f+g^2\overset{\text{I}}{x}^2\right)$$
$$+\frac{\overset{\text{I}}{A}^2\cdot\overset{\text{II}}{A}^2}{\overset{\text{I}}{B}^2\cdot B^2\cdot\overset{\text{II}}{P}}\left(\frac{\overset{\text{II}}{P}}{\overset{\text{II}}{A}+\overset{\text{II}}{B}}+f+g^2\overset{\text{II}}{x}^2\right)=0.$$

Symbola hanc æquationem ingredientia per hypotheſin hujus exempli ita determinantur: ob A infinitam eſt $\frac{P}{A+B}=0$, et $B=P$. Ob $\overset{\text{II}}{B}$ infinitam eſt $\frac{\overset{\text{II}}{P}}{\overset{\text{II}}{A}+\overset{\text{II}}{B}}=0$, et $\overset{\text{II}}{A}=\overset{\text{II}}{P}$. Ob lentes P et $\overset{\text{I}}{P}$ juxta ſe poſitas; eſt earum diſtantia $\overset{\text{I}}{A}+B=0$, ſive $\overset{\text{I}}{A}=-B=-P$. Ob $\frac{\text{I}}{R}=\frac{\text{I}}{P}+\frac{\text{I}}{\overset{\text{I}}{P}}$, et $\overset{\text{I}}{P}=nP$, eſt $P=\frac{n+1}{n}R$, et $\overset{\text{I}}{P}=\overline{n+1}.R$. Præterea eſt $\overset{\text{I}}{B}=R$, et $\overset{\text{II}}{P}=\frac{R}{m}$. His itaque valoribus ſubſtitutis, et æquatione ordinata, habetur relatio indicum x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, expreſſa ut ſequitur: $g^2\times(n^3x^2+\overset{\text{I}}{x}^2+\frac{\overline{n+1}^3}{m}\overset{\text{II}}{x}^2)$ $+f\times(n^3+1+\frac{\overline{n+1}^3}{m})=n.\overline{n+1}$. Determinatis indicibus x, $\overset{\text{I}}{x}$, $\overset{\text{II}}{x}$, habentur per § 11. radii facierum: videlicet,

Radius faciei anterioris lentis primæ $=\frac{\overline{n+1}.R}{n.\overline{b+x}}$, poſterioris $=\frac{\overline{n+1}.R}{n.\overline{K-x}}$;

Radius faciei anterioris lentis ſecundæ $=\frac{\overline{n+1}.R}{\overline{n+1}.b-nK+\overset{\text{I}}{x}}$, poſterioris $=\frac{\overline{n+1}.R}{\overline{n+1}.K-nb-\overset{\text{I}}{x}}$;

Radius

Radius faciei anterioris lentis tertiæ $= \frac{R}{m \cdot K + \overset{II}{x}}$,

posterioris $= \frac{R}{m \cdot b - \overset{II}{x}}$.

Ita si vitrum objectivum hujus telescopii componendum sit ex duabus lentibus, convexa et concava, quarum distantiæ focales sint ut 3 et 2, debeatque primo lens convexa anterior collocari, oportebit sumere $n = -\frac{3}{2}$, et erit æquatio pro indicibus:

$$g^2 \times \left(27 x^2 - 8 \overset{I}{x^2} + \frac{\overset{II}{x^2}}{m}\right) + f \times \left(19 + \frac{I}{m}\right) + 6 = 0,$$

et radii facierum:

Radius faciei anterioris lentis primæ $= \frac{R}{3b + 3x}$,

posterioris $= \frac{R}{3K + 3x}$;

Radius faciei anterioris lentis secundæ $= -\frac{R}{3K - b + 2\overset{I}{x}}$, posterioris $= -\frac{R}{3b - K - 2\overset{I}{x}}$;

Radius faciei anterioris lentis tertiæ $= \frac{R}{mK + m\overset{II}{x}}$,

posterioris $= \frac{R}{mb - m\overset{II}{x}}$.

Si vero lens concava anterior est collocanda, debet sumi $n = -\frac{2}{3}$, unde æquatio pro indicibus prodit

$$g^2 \times \left(-8 x^2 + 27 \overset{I}{x^2} + \frac{\overset{II}{x^2}}{m}\right) + f \times \left(19 + \frac{I}{m}\right) + 6 = 0,$$

et consequenter:

Radius faciei anterioris lentis primæ $= -\frac{R}{2b + 2x}$,

posterioris $= -\frac{R}{2K - 2x}$;

Radius

Radius faciei anterioris lentis ſecundæ $= \frac{R}{b + 2K + 3x^{I}}$, poſterioris $= \frac{R}{K + 2b - 3x^{I}}$;

Radius faciei anterioris lentis tertiæ $= \frac{R}{mK + mx^{II}}$, poſterioris $= \frac{R}{mb - mx^{II}}$.

§ 15.

Vidimus in ſolutione generati noviſſimi problematis (§ ii.) et ſubjunctis exemplis (§ 12, 13, 14.) relationem indicum x, x^{I}, x^{II}, &c. unica tantum æquatione definiri, adeoque judicio artificis plurimum eſſe relictum in commoda eorum aſſumtione facienda. Duæ autem ſunt conſiderationes, quibus hæc aſſumtio utcunque dirigi poſſit. Prima eſt, ut nullus indicum fiat nimis magnus, ſive, quod eodem recidit, ut formæ lentium quamproxime accedant ad eas, quæ in datis circumſtantiis aberrationem dant omnium minimam. Hac enim ratione errores in forma lentium forte commiſſi minus nocebunt, quemadmodum ex natura minimi notum eſt. Altera eſt, ut ſingulæ lentes, quam fieri poteſt, proxime evadant æqualiter utrinque convexæ, vel æqualiter concavæ, quo fiet, ut majores aperturæ ſint patientes. Quantitates enim aberrationum ſupra definitæ non ſunt niſi quamproxime veræ, et tanto magis fallunt, quanto facies lentium fuerint majora ſuarum ſphærarum ſegmenta. Binæ hæ regulæ, etiamſi ſibi invicem ſæpius adverſuntur, nec facile definiri queat, quantum in quovis dato caſu uni vel alteri ipſarum ſit tribuendum; non dubito tamen, quin judicioſo artifici ſint

profuturæ

profuturæ ad mediam viam tenendam inter utramque.

Eædem quoque conſiderationes impediunt, quo minus certi aliquid definire auſim, de maximo poſſibili effectu inſtrumentorum opticorum ſecundum hæc principia compoſitorum, vel de modo perfectiſſima componendi. Regulæ huc pertinentes tutiſſime exſpectantur ab experientia, modo dirigatur à theoria.

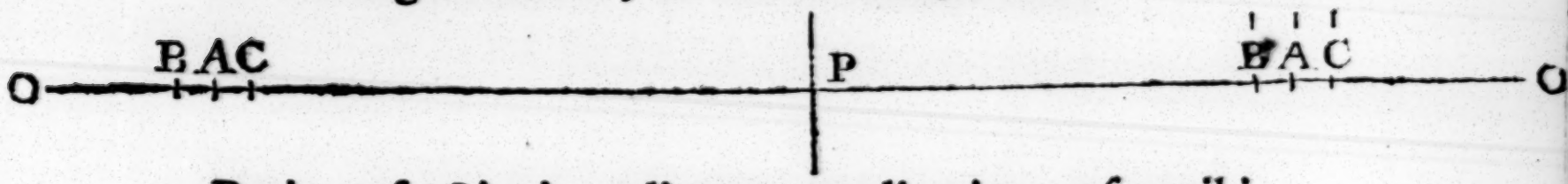
§ 16.

Expoſitis jam breviter quæ dicenda habuimus de aberrationibus radiorum homogeneorum refractorum, ortis a lentium figura ſphærica, ut et de modo illas corrigendi in inſtrumentis opticis: liceat etiam pauca addere de altera illa aberrationis ſpecie, quæ a diverſa radiorum refrangibilitate oritur, quæ objecta trans lentes viſa coloribus inficit, et maximam confuſionis partem producit. Creditum hucuſque communiter fuit hoc vitium nulla arte emendabile, donec ingenioſiſſimus artifex Londinenſis, Dollandus, experimentis inſtitutis feliciter deprehenderet, varia dari vitrorum genera, quæ, licet fere æqualiter refringant certum quoddam radiorum genus, plurimum tamen diſcrepent viribus refringendi reliqua radiorum genera, adeoque et radios heterogeneos a ſe invicem ſeparandi: quo eximio invento et theoriam et praxim optices inſigniter auxit. Diſcimus enim hinc, rationes refractionem radiorum diverſi generis nullo modo a ſe invicem dependere, ut hucuſque fuit creditum; adeoque fruſtra quæri regulam, qua ex datis refractionibus radiorum diverſi generis in uno aliquo medio inveniantur refractiones eorumdem in alio; vel

vel qua ex data refractione unius alicujus radii in quovis medio, inveniuntur refractiones reliquorum in eodem medio. Praxis quoque teleſcopiorum dioptricorum inſigne incrementum hujus inventi beneficio nacta eſt. Scilicet ſagaciſſimus inventor modum inde derivavit corrigendi aberrationes ex ſeparatione radiorum heterogeneorum oriundas, componendo vitrum objectivum ex duabus lentibus, una convexa, altera concava, quæ ita ſint comparatæ, ut una alterius effectum in radiis ſeparandis deſtruat, quemadmodem in Vol. L. Part. II. p. 733. et ſeq. Tranſactionum Philoſophicarum expoſuit. Quod artificium ita breviter explicabimus.

§ 17.

Sit P lens quæcunque, habens radios facierum ſuarum *a* et *b*; diſtantiam foci principalis mediocriter refrangibilium P; et *a x* in rectam O O.

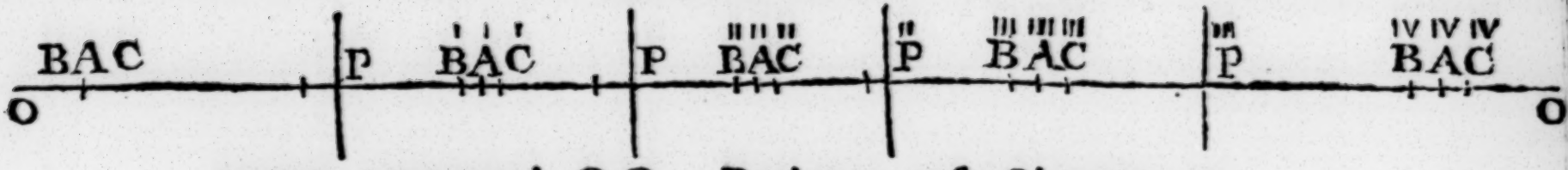


Ratio refractionis radiorum mediocriter refrangibilium ex aëre in lentem ſit N ad 1; radiorum maxime refrangibilium N + *n* ad 1, et radiorum minime refrangibilium N — *n* ad 1, exiſtente *n* numero valde parvo reſpectu ipſius N. In axe lentis O O ſint puncta A, B, C foci radiorum in lentem P incidentium, A mediocriter refrangibilium, B maxime et C minime refrangibilium, quorum focorum diſtantiæ A B, A C ſint valde parvæ, et ponantur puncta $\overset{I}{A}$, $\overset{I}{B}$, $\overset{I}{C}$, foci conjugati ipſorum A, B, C reſpective, pro ſua quique radiorum ſpecie, ita ut ſint B C, $\overset{I}{B}\overset{I}{C}$,

diſſipationes radiorum heterogeneorum circa focos conjugatos A et $\overset{\text{I}}{\text{A}}$. Quæratur earum relatio.

Quoniam B et $\overset{\text{I}}{\text{B}}$ ſunt foci conjugati radiorum maxime refrangibilium, quorum ratio refractionis eſt $\text{N} + n$ ad 1, erit $\frac{1}{\text{PB}} + \frac{1}{\overset{\text{I}}{\text{PB}}} = \overline{\text{N} + n - 1} \cdot \overline{\frac{1}{a} + \frac{1}{b}}$. Similiter quoniam puncta C et $\overset{\text{I}}{\text{C}}$ ſunt foci conjugati radiorum minime refrangibilium, quorum ratio refractionis eſt $\text{N} - n$ ad 1, erit $\frac{1}{\text{PC}} + \frac{1}{\overset{\text{I}}{\text{PC}}} = \overline{\text{N} - n + 1} \cdot \overline{\frac{1}{a} + \frac{1}{b}}$. Subtrahatur hæc poſterior æquatio à priore, et habebitur $\frac{1}{\text{PB}} - \frac{1}{\text{PC}} + \frac{1}{\overset{\text{I}}{\text{PB}}} - \frac{1}{\overset{\text{I}}{\text{PC}}} = 2n \cdot \overline{\frac{1}{a} + \frac{1}{b}}$, ſive conjungendo binos terminos: $\frac{\text{PC} - \text{PB}}{\text{PB} \times \text{PC}} + \frac{\overset{\text{I}}{\text{PC}} - \overset{\text{I}}{\text{PB}}}{\overset{\text{I}}{\text{PB}} \times \overset{\text{I}}{\text{PC}}} = 2n \cdot \overline{\frac{1}{a} + \frac{1}{b}}$, quæ pro $\text{PB} \times \text{PC}$ et $\overset{\text{I}}{\text{PB}} \times \overset{\text{I}}{\text{PC}}$, ſcribendo $\text{PA}\,q$ et $\overset{\text{I}}{\text{PA}}\,q$, et pro $\frac{1}{a} + \frac{1}{b}$ ſcribendo $\frac{1}{\overline{\text{N} - 1} \cdot \text{P}}$, evadit $\frac{\overset{\text{I}}{\text{BC}}}{\overset{\text{I}}{\text{PA}}\,q} = \frac{2n}{\overline{\text{N} - 1} \cdot \text{P}} + \frac{\text{BC}}{\text{PA}\,q}$, exhibens relationem inter diſſipationes conjugatas BC et $\overset{\text{I}}{\text{BC}}$.

His præmiſſis, intelligantur lentes quotcunque P, $\overset{\text{I}}{\text{P}}$, $\overset{\text{II}}{\text{P}}$, $\overset{\text{III}}{\text{P}}$, &c. quarum diſtantiæ focorum principalium reſpectu radiorum mediocriter refrangibilium ſint P, $\overset{\text{I}}{\text{P}}$, $\overset{\text{II}}{\text{P}}$, $\overset{\text{III}}{\text{P}}$, &c. reſpective, ordine diſpoſitæ in axe earum



earum communi O O. Rationes refractionum radiorum mediocriter refrangibilium in his lentibus ordine ſumtis ſint reſpective, N ad 1, $\overset{I}{N}$ ad 1, $\overset{II}{N}$ ad 1, $\overset{III}{N}$ ad 1, &c. radiorum maxime refrangibilium $N + n$ ad 1, $\overset{I}{N} + \overset{I}{n}$ ad 1, $\overset{II}{N} + \overset{II}{n}$ ad 1, $\overset{III}{N} + \overset{III}{n}$ ad 1, &c. et radiorum minime refrangibilium $N - n$ ad 1, $\overset{I}{N} - \overset{I}{n}$ ad 1, $\overset{II}{N} - \overset{II}{n}$ ad 1, $\overset{III}{N} - \overset{III}{n}$ ad 1, &c. Puncta A, $\overset{I}{A}$, $\overset{II}{A}$, $\overset{III}{A}$, $\overset{IV}{A}$, &c. ſint foci conjugati lentium reſpectu radiorum mediocriter refrangibilium, puncta B, $\overset{I}{B}$, $\overset{II}{B}$, $\overset{III}{B}$, $\overset{IV}{B}$, &c. reſpectu radiorum maxime refrangibilium, C, $\overset{I}{C}$, $\overset{II}{C}$, $\overset{III}{C}$, $\overset{IV}{C}$, &c. reſpectu minime refrangibilium, ita ut BC, $\overset{I\ I}{B\,C}$, $\overset{II\ II}{B\,C}$, $\overset{III\ III}{B\,C}$, $\overset{IV\ IV}{B\,C}$, &c. ſint diſſipationes ſucceſſivæ radiorum heterogeneorum in axe lentium. Dicantur tandem diſtantiæ focorum conjugatorum à ſuis reſpective lentibus: PA = A, $\overset{I}{P}A = B$; $\overset{I\ I}{P\,A} = A$, $\overset{I\ II}{P\,A} = B$; $\overset{II\ II}{P\,A} = A$, $\overset{II\ III}{P\,A} = B$; $\overset{III\ III}{P\,A} = A$, $\overset{III\ IV}{P\,A} = B$, &c.

Jam ſi in æquatione pro relatione diſſipationum conjugatarum modo inventa, $\frac{\overset{I\ I}{B\,C}}{\overset{I}{P}A^q} = \frac{2n}{N-1.P} + \frac{BC}{PA^q}$, per BC et $\overset{I\ I}{B\,C}$ intelligantur ſucceſſive ſingula paria diſſipationum conjugatorum, et per PA et $\overset{I}{P}A$ diſtantiæ focorum conjugatorum ipſis reſpondentes; item

item per P, N et n eorum valores ſingulis lentis proprii, habebuntur tot æquationes, quot ſunt lentes: videlicet; $\overset{\text{I}}{\text{B}}\overset{\text{I}}{\text{C}} = \text{B}^2 \times \left(\frac{2n}{\text{N}-1 \,.\, \text{P}} + \frac{\text{BC}}{\text{A}^2}\right)$; $\overset{\text{II}}{\text{B}}\overset{\text{II}}{\text{C}} = \overset{\text{I}}{\text{B}}{}^2 \times \left(\frac{2\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1 \,.\, \overset{\text{I}}{\text{P}}} + \frac{\overset{\text{I}}{\text{B}}\overset{\text{I}}{\text{C}}}{\overset{\text{I}}{\text{A}}{}^2}\right)$; $\overset{\text{III}}{\text{B}}\overset{\text{III}}{\text{C}} = \overset{\text{II}}{\text{B}}{}^2 \times \left(\frac{2\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1 \,.\, \overset{\text{II}}{\text{P}}} + \frac{\overset{\text{II}}{\text{B}}\overset{\text{II}}{\text{C}}}{\overset{\text{II}}{\text{A}}{}^2}\right)$; $\overset{\text{IV}}{\text{B}}\overset{\text{IV}}{\text{C}} = \overset{\text{III}}{\text{B}}{}^2 \times \left(\frac{2\overset{\text{III}}{n}}{\overset{\text{III}}{\text{N}}-1 \,.\, \overset{\text{III}}{\text{P}}} + \frac{\overset{\text{III}}{\text{B}}\overset{\text{III}}{\text{C}}}{\overset{\text{III}}{\text{A}}{}^2}\right)$; et ita porro, ſi plures fuerint lentes. Ponantur puncta B, A, C, coincidere, ſive ex primo foco A prodire radium compoſitum, ut diſſipatio prima BC ſit nulla, et reducendo has æquationes, ponendo $\text{BC} = 0$, habentur valores diſſipationum ſucceſſivarum, pro quovis lentium numero:

1. $\overset{\text{I}}{\text{B}}\overset{\text{I}}{\text{C}} = \text{B}^2 \times \frac{2n}{\text{N}-1 \,.\, \text{P}}$.

2. $\overset{\text{II}}{\text{B}}\overset{\text{II}}{\text{C}} = \overset{\text{I}}{\text{B}}{}^2 \times \frac{2\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1 \,.\, \overset{\text{I}}{\text{P}}} + \frac{\overset{\text{I}}{\text{B}}{}^2 \,.\, \text{B}^2}{\overset{\text{I}}{\text{A}}{}^2} \times \frac{2n}{\text{N}-1 \,.\, \text{P}}$.

3. $\overset{\text{III}}{\text{B}}\overset{\text{III}}{\text{C}} = \overset{\text{II}}{\text{B}}{}^2 \times \frac{2\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1 \,.\, \overset{\text{II}}{\text{P}}} + \frac{\overset{\text{II}}{\text{B}}{}^2 \,.\, \overset{\text{I}}{\text{B}}{}^2}{\overset{\text{II}}{\text{A}}{}^2} \times \frac{2\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1 \,.\, \overset{\text{I}}{\text{P}}} + \frac{\overset{\text{II}}{\text{B}}{}^2 \,.\, \overset{\text{I}}{\text{B}}{}^2 \,.\, \text{B}^2}{\overset{\text{II}}{\text{A}}{}^2 \,.\, \overset{\text{I}}{\text{A}}{}^2} \times \frac{2n}{\text{N}-1 \,.\, \text{P}}$.

4. $\overset{\text{IV}}{\text{B}}\overset{\text{IV}}{\text{C}} = \overset{\text{III}}{\text{B}}{}^2 \times \frac{2\overset{\text{III}}{n}}{\overset{\text{III}}{\text{N}}-1 \,.\, \overset{\text{III}}{\text{P}}} + \frac{\overset{\text{III}}{\text{B}}{}^2 \,.\, \overset{\text{II}}{\text{B}}{}^2}{\overset{\text{III}}{\text{A}}{}^2} \times \frac{2\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1 \,.\, \overset{\text{II}}{\text{P}}} + \frac{\overset{\text{III}}{\text{B}}{}^2 \,.\, \overset{\text{II}}{\text{B}}{}^2 \,.\, \overset{\text{I}}{\text{B}}{}^2}{\overset{\text{III}}{\text{A}}{}^2 \,.\, \overset{\text{II}}{\text{A}}{}^2} \times \frac{2\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1 \,.\, \overset{\text{I}}{\text{P}}} + \frac{\overset{\text{III}}{\text{B}}{}^2 \,.\, \overset{\text{II}}{\text{B}}{}^2 \,.\, \overset{\text{I}}{\text{B}}{}^2 \,.\, \text{B}^2}{\overset{\text{III}}{\text{A}}{}^2 \,.\, \overset{\text{II}}{\text{A}}{}^2 \,.\, \overset{\text{I}}{\text{A}}{}^2 \,.\, \text{A}^2} \times \frac{2n}{\text{N}-1 \,.\, \text{P}}$.

Et ita deinceps, ſi plures fuerint lentes.

§ 18.

§ 18.

Datis positionibus lentium quotcunque in communi axe, una cum singularum legibus refractionum pro radiis omnis generis, oporteat invenire relationem inter distantias focorum principalium earundem lentium, quæ efficiat, ut radii heterogenei à quovis puncto advenientes vel paralleli, post refractionem in omnibus lentibus emergant, sine diffipatione à diversa refrangibilitate radiorum oriunda, si fieri potest.

Solutio problematis generaliter absolvitur ponendo expressionem diffipationis, datæ multitudini lentium convenientem, § 17, nihilo æqualem, quo ipso habetur æquatio definiens relationum distantiarum focalium, P, $\overset{\text{I}}{P}$, $\overset{\text{II}}{P}$, $\overset{\text{III}}{P}$, &c. requisitam ad id, ut diffipatio in foco ultimo evanescat. Ita in casu binarum lentium habebitur: $\frac{\overset{\text{I}}{n}}{\overset{\text{I}}{N}-1\,.\,\overset{\text{I}}{P}} + \frac{B^2}{\overset{\text{I}}{A^2}} \times \frac{n}{N-1\,.\,P} = 0$;

in casu trium lentium erit: $\frac{\overset{\text{II}}{n}}{\overset{\text{II}}{N}-1\,.\,\overset{\text{II}}{P}} + \frac{\overset{\text{I}}{B^2}}{\overset{\text{II}}{A^2}} \times \frac{\overset{\text{I}}{n}}{\overset{\text{I}}{N}-1\,.\,\overset{\text{I}}{P}} + \frac{\overset{\text{I}}{B^2}\,.\,B^2}{\overset{\text{II}}{A^2}\,.\,\overset{\text{I}}{A^2}} \times \frac{n}{N-1\,.\,P} = 0$; in casu quatuor lentium:

$$\frac{\overset{\text{III}}{n}}{\overset{\text{III}}{N}-1\times\overset{\text{III}}{P}} + \frac{\overset{\text{II}}{B^2}}{\overset{\text{III}}{A^2}} \times \frac{\overset{\text{II}}{n}}{\overset{\text{II}}{N}-1\times\overset{\text{II}}{P}} + \frac{\overset{\text{II}}{B^2}\,.\,\overset{\text{I}}{B^2}}{\overset{\text{III}}{A^2}\,.\,\overset{\text{II}}{A^2}} \times \frac{\overset{\text{I}}{n}}{\overset{\text{I}}{N}-1\times\overset{\text{I}}{P}} + \frac{\overset{\text{II}}{B^2}\,.\,\overset{\text{I}}{B^2}\,.\,B^2}{\overset{\text{III}}{A^2}\,.\,\overset{\text{II}}{A^2}\,.\,\overset{\text{I}}{A^2}} \times \frac{n}{N-1\times P} = 0;$$

et ita porro, si plures fuerint lentes.

Ita

Ita ſi in æquatione pro binis lentibus, $\frac{\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}} - 1 \,.\, \overset{\text{I}}{\text{P}}}$ $+ \frac{\text{B}^2}{\overset{\text{I}}{\text{A}^2}} \times \frac{n}{\text{N} - 1 \,.\, \text{P}} = 0$, ponatur diſtantia lentium $\text{B} + \overset{\text{I}}{\text{A}} = 0$, ut evadant contiguæ, quemadmodum in vitro objectivo Dollondiano; habetur $\frac{\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}} - 1 \,.\, \overset{\text{I}}{\text{P}}}$ $+ \frac{n}{\text{N} - 1 \,.\, \text{P}} = 0$. Unde patet alterutram lentium P et $\overset{\text{I}}{\text{P}}$ faciendam eſſe concavam, alteram convexam, et diſtantias earum focales affirmative ſumtas P et $\overset{\text{I}}{\text{P}}$ faciendas eſſe in ratione $\frac{n}{\text{N} - 1}$ ad $\frac{\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}} - 1}$, quæ ratio evadit n ad $\overset{\text{I}}{n}$, ſi vires refractivæ mediæ utriuſque lentis fuerint æquales.

Similiter ſit conſtruendum teleſcopium ex tribus lentibus, quarum duæ priores P et $\overset{\text{I}}{\text{P}}$ juxta ſe poſitæ conſtituant vitrum objectivum, tertia $\overset{\text{II}}{\text{P}}$ ſit ocularis, et potentia amplificandi exprimatur numero m. Quoniam lentes priores P et $\overset{\text{I}}{\text{P}}$ ſunt juxta ſe poſitæ, erit earum diſtantia $\text{B} + \overset{\text{I}}{\text{A}} = 0$. Quoniam per conſtitutionem teleſcopii radii in primam lentem incidentes ſunt paralleli, erit $\text{B} = \text{P}$, et $\frac{\text{I}}{\overset{\text{I}}{\text{B}}} = \frac{\text{I}}{\text{P}} + \frac{\text{I}}{\overset{\text{I}}{\text{P}}}$. Quoniam radii ex ultima lente emergunt paralleli, erit $\overset{\text{II}}{\text{A}} = \overset{\text{II}}{\text{P}}$. Et tandem, quia potentia amplificandi

 indicatur

indicatur numero m, erit $\overset{\text{I}}{\text{B}} = m\overset{\text{II}}{\text{P}}$. Subſtitutis itaque his valoribus in æquatione pro tribus lentibus ſupra allata, tranſit illa in hanc: $\frac{n}{\text{N}-1\,.\,\text{P}} + \frac{\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1\,.\,\overset{\text{I}}{\text{P}}} + \frac{\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1\,.\,m^2\overset{\text{II}}{\text{P}}} = 0$. Et hæc æquatio una cum æquatione $\frac{\text{I}}{\text{P}} + \frac{\text{I}}{\overset{\text{I}}{\text{P}}} - \frac{\text{I}}{m\overset{\text{II}}{\text{P}}} = 0$, determinat rectationem diſtantiarum focalium P, $\overset{\text{I}}{\text{P}}$, $\overset{\text{II}}{\text{P}}$, qualis ad id requiritur, ut radiorum emergentium diſſipatio à diverſa refrangibilitate nulla ſit. Comparando ſcilicet has æquationes, habetur $\left(\frac{nm}{\text{N}-1} + \frac{\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1}\right)\overset{\text{I}}{\text{P}} + \left(\frac{\overset{\text{I}}{nm}}{\overset{\text{I}}{\text{N}}-1} - \frac{\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1}\right)\text{P} = 0$, et $\left(\frac{n}{\text{N}-1} - \frac{\overset{\text{I}}{n}}{\overset{\text{I}}{\text{N}}-1}\right) \times m^2\overset{\text{II}}{\text{P}} + \left(\frac{\overset{\text{I}}{nm}}{\overset{\text{I}}{\text{N}}-1} + \frac{\overset{\text{II}}{n}}{\overset{\text{II}}{\text{N}}-1}\right) \times \text{P} = 0$. Eadem ratione procedendum eſt in aliis caſibus.

S. Klingenſtierna.

Stockholm,
20 Aprilis 1760.

ERRATUM.

Page 900, Line 28, *after* as uſual *add* and the poker *plus.*

AN

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TO THE

FIFTY-FIRST VOLUME

OF THE

Philoſophical Tranſactions.

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